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ORIGINAL COMMUNICATIONS.

*On the Functions of the Great Sympathetic Nerve, or the Nerve of Organic Life.* By WM. GRIES, M. D., of the city of Reading, Pennsylvania.

In an essay, which I had the honor of reading before the "Medical Society of the city of Reading, and county of Berks," on the 6th of April last, and again before the "Medical Society of the State of Pennsylvania," at its annual session in the last week in May, at Philadelphia, I made the following cursory remark: "Upon serious reflection I looked upon all the diseases as being under the influence of some cause affecting more particularly the organic nervous system, prostrating it, and disturbing its proper action—and I concluded that this must be corrected—and it appeared to me that the sulphate of quinine, in proper doses, would do this better than any other medicine." If I am not deficient in information, the opinion held by the profession in general, with reference to the functions of the organic nerve, is, that it may be compared to lines of telegraphic wires extended from one organism to another, to give mutual information how each performs its functions, and to combine all the organisms

in a concert of action, to work out the great objects of manifesting life. This I have been led to consider a very imperfect and partial view: and I have been perfectly astounded that the views brought out, more than thirty years ago, by Dr. Jas. Copland, of England, and so ably extended and maintained in his great work, "the Dictionary of Practical Medicine," should have attracted so little attention from the profession. The more I have studied the subject in his writings and by my own observations upon the diseases that have come under my notice, the more have I been convinced of their truthfulness; and I refer with confidence to the cases recorded in my essay above referred to, as illustrative and confirmatory of those views. Whilst I was making those observations, I was perfectly electrified when I read, in the April number of the "American Journal of the Medical Sciences" the experiments performed on the great sympathetic nerve, by M. Claude Bernard; and before I had finished the reading of that short notice, I was irresistibly convinced that it fully confirmed all those views of Dr. Copland. I reasoned thus: if the destruction of a small cervical branch of the sympathetic has the effect of producing those results stated in that notice, it must, of necessity, follow, that an impairment of its vitality must, in a degree commensurate with the degree of that impairment, be followed by similar results on the whole system. I think that I may safely say, that I have now treated more than one hundred cases, similar to those recorded in my essay above referred to, upon the same plan, and I hesitate not to say, that, without a single exception, with equal success. In the observations I have made upon those cases I have much extended Copland's views, as far as their record has come under my review. I now look upon the great sympathetic as God's prime minister, appointed to preside over the whole organization of man, and to actuate and regulate all its vital actions, both physical and mental.

I believe that the nervous system of animal life, is as much under its influence and guidance for the performance of its proper functions, as any other organism in the body. In further illustration of my views I am tempted to say, with trembling awe, fearing that it borders on blasphemy, that the combined nervous systems are a triune government; that the great sympathetic operates through the system of animal life, and emanating from



both, the nervous fluid, or whatever name I may give it, manifests its power and action on the organism.

In illustration and confirmation of my views, which I have so categorically stated, I shall now recite other cases, and make observations upon them. As I have been myself the chief subject of experiment, I must now, even at the risk of being accused of egotism, give something of my personal history. However, I feel confident that the true physiological enquirer, for whom alone I write, will clear me of such an imputation.

About forty six years ago, when I was ten years of age, I had the intermittent fever for three months, of a tertian form. I could not retain sufficient cinchona on my stomach to interrupt or put a stop to the paroxysms, and my physician gave me frequent emetics without attaining that end; and I began to hate him and his emetics; for the sight of him was almost enough to make me vomit. I remember well yet, that in the latter part of my sufferings, when the paroxysm was at its height, I would have a sensation as if my head was expanding, and at last filling the whole house that I was in; when, of a sudden, I would be transported to the edge of a precipice or a chasm, and in the act of toppling over, I would scream out in agony; and my father would lead me or carry me about, which seemed to relieve me. From that time onward, all my mental faculties seemed to be blunted. My memory was less retentive; I was stupid and drowsy. I would frequently, when a grown up young man, fall asleep in the midst of company, even that of young ladies. I tried various plans to counteract it—low diet, &c.—but all without effect. Notwithstanding this impairment of my mental faculties, I never lost my appetite for mental food. I was mentally omnivorous. I thought frequently of Bacon's Aphorism, "reading makes a full man, writing a correct man, and conversation a ready man," and I often resolved to be governed by it, but never succeeded. I would add to the first branch—it also makes a dull and stupid man—for repletion with mental food has the same stupifying effect, that repletion has upon our physical organization. That I never benefitted by the second is abundantly evident, from the specimen before the reader. I have no power for precision in language; consequently I employ figures or analogies for illustration. I take hold of a figure, but I don't carry it out, and as

I pass on I fling away one and grasp at another, as it comes readily to hand. I have written but little, and, with the unfaithful steward, I buried my one talent, and took credit to myself for doing so. With reference to the last branch, I had but little opportunity to benefit myself, until the formation of the American Medical Association, and its auxiliaries.

I tried various means to excite my mental faculties ; all alcoholic stimulants, distilled or fermented, always rendered me more stupid. When phrenology came in vogue, I paid considerable attention to the subject, and came to the conclusion that my brain, being a very large one, was made of very coarse material, and was never intended for literary or scientific occupation ; and the rest of my physical structure being of little strength, I was led to fear that I was but poorly fitted for any station. As regards the phrenological development of some of the organs, veneration is very deficient, so that it requires a continual effort to properly venerate the Author of my being ; but firmness, or what in me might, perhaps, more correctly be called stubbornness, is very strongly developed. Now the subject is described ; next come the experiments.

At an early period of the prevalence of the peculiar state of disease described in the essay referred to above, I found my health deteriorating ; my hepatic secretions and my bowels were particularly affected ; it seemed paroxysmal, generally worse late in the afternoon and night. On account of being, for many years, a sufferer from emphysema of the lungs, I feared the use of quinia in large doses. I used mercurial alteratives and mild laxatives, to mend the secretions, but they would not be mended. My appetite was impaired, my strength failed, and I was unapt for mental or physical exertion. I determined at length, by observing the benefit resulting on others from large doses of quinine, to use it on myself. I commenced by taking ten grains in the evening, before going to bed ; it was followed, in about one hour, by a slight tremor in my hands, and in fact in all the voluntary muscles ; but what completely and agreeably surprised me, and what I had not, in the smallest degree, anticipated, was the clearness and expansion of my mental faculties ; I could think more clearly ; and, after the lapse of about two hours, I fell asleep ; and slept more soundly than I had for many years—(for



although I was always drowsy more or less, unless engaged in bodily exercise, yet I never slept soundly, and my awakening was not sudden.) I felt much better in the morning, more inclined to attend to my duties, and more capable of attention.

Upon reflection, through the day, I concluded that I had taken an overcharge (like an overcharge of an electric battery,) and I took a diminished dose in the evening. It was followed by less tremor, but equally sound sleep and readiness for mental and bodily action. In a few days I brought the dose to five grains, and took it before breakfast in the morning: at this dose it produced no tremor, and was followed by all the good desired. I have been taking it regularly now for four months, with an occasional interruption of two or three days. My appetite has been good, my secretions are corrected, my mind and body are invigorated; I have had no occasion for alteratives or purgatives. In a word, I have enjoyed life more for the last four months, than I had done for forty-six years. I had for a considerable time held the view that the remote cause of ague operated upon the great sympathetic, impairing, in some manner, its functions, and that this impairment was the proximate cause of ague; now I feel perfectly convinced of its truth, and, in my own case, reason: that my protracted ague, at ten years of age, impaired the power of the sympathetic, and that the sulph. quiniæ has restored that power. I further believe that that terrific disease, tetanus, may reasonably be ascribed to the impairment of the superintending power of the sympathetic over the nervous system of animal life, and that the latter runs furiously wild without the controlling check of the former. And I further believe that in very large doses of quinia, frequently repeated, will be found the remedy for this opprobrium of medicine. Nay, I go still further, to believe that even hydrophobia will yet submit to it. I feel that I am riding a hobby, and I am terrified when I reflect that many men of much superior capacity and experience have been engulfed in the slough and quagmire, by not restraining their hobbies; yet I cannot find prudence enough to draw in the reins of mine; his gait is so pleasant, I cannot look down, but up, and under a charm I go ahead. I write this only, as it were, to call for help, that if my friends see my danger they may cry out and stop me.

I hope that no one will understand me to say, that the sulph. quin. is a remedy for all kinds of morbid action in the sympathetic. It is intended only in its dynamic action. It is only the remedy when it is in an adynamic state, to raise it to its proper standard, and to re-establish it in its proper office. Thus far my experience warrants me. That other morbid states may arise requiring different therapeutic agents, I have no doubt; but I do not understand them. My main object is to draw attention to its functions and to excite inquiry.

I have found it necessary to use more tonics of all sorts during the last six months than I did during ten years before, in proportion to the number of patients. Indeed, twenty-five years ago I was inexperienced enough to believe, that tonics might be expunged from the list of therapeutic agents. In order to show the perfect safety of the large doses of quinine, spoken of in my former essay, I will state, rather minutely, the case of a little girl, now two years of age. She was from birth of a delicate constitution, always very restless. About six months ago, when I was called to see her, she had pretty well developed symptoms of inflammation of the brain:—I got her over it with difficulty, and she had a protracted convalescence. During an early period of the epidemic scarlatina of last winter, she was attacked by that disease, and had a slow convalescence. About six weeks ago, she was taken with a severe attack of measles.

I ought to have mentioned that she was, almost from birth, affected by a slight cough. After the measles passed off, she was weak, fretful and sleepless. I had to give her opiates, and then again alteratives, to do away the mischief that the opium had done. I thought in her convalescence from scarlet fever that she was affected by the general epidemic influence, and I gave her, several times, two grain doses of quinia, with benefit.

During the protracted recovery from measles, I gave, on the 21st of May, four grains of sulph. quiniæ, in the evening; next morning the mother reported, that she had slept better that night, than she had for many nights before, and awoke much more cheerful. I gave her again four grains of quinia, and the next morning her mother remarked, when I called to see her, "Doctor, it is very strange that Amelia, always, when she takes that white medicine, sleeps better than on anything else, and awakes more



cheerful. On the 24th of May, as I was preparing to go to Philadelphia, I left a mixture of four grains of sulph. quin. to be given on the 28th, to prevent a relapse. Last evening her mother sent for some drops, to make her sleep, and I sent the four grain quinia mixture; this morning (June 4th,) I found she had slept well on it, and was very cheerful. I think it not out of place here to state another very remarkable case. On the 10th of April, I was called to a lady, who had born, three children, at single births, who are all living. She had been sick five days, under the hands of another *doctor*. I found her vomiting blood largely; she had great tenderness over the epigastrium—a costive state of bowels. She had also occasional bleeding from the nose. She thought that she might be two months enceinte. Her tongue, when protruded, was contracted, pointed, and of a deep red. Her *doctor* had given her laudanum and ether for her pain! and hot strong cinnamon tea to stop vomiting! According to her account she had an hemorrhagic diathesis from her youth up.

I commenced my treatment by interdicting all that had been done; and ordered cups to the epigastrium, and an enema of turpentine and castor oil. I directed them to saturate lemon juice with table salt, and to put a teaspoonful of this into half a pint of iced water, and to use this altogether for her drink. She immediately improved on this; but the case proved exceedingly protracted and intractable. On the 1st of May, I tried nitr. argenti with opium, but I had soon to desist, for it irritated the stomach. I then tried creasote in small doses, but it would not answer. On the fourth of May I determined to use the sulph. quiniæ, because I had observed for several days a paroxysm every afternoon, manifesting itself by a little coldness of the knees, and, crampy pain of the stomach, with distressing nausea and vomiting of blood; but her stomach would not tolerate the quinine. I then made up my mind to introduce the quinine, in large doses, into the rectum on the sixth of May. But early in the morning her husband came into my office, to tell me that she insisted upon having a homœopathic doctor. He attended her three days with great promises but no performance; when they applied to one of our old physicians, of the old school. On the 15th of May, they begged of me to take the case again in hand. Upon visiting her I

found her exceedingly prostrated and emaciated. I immediately proceeded to carry out my project; and from the 15th to the 21st of May, I had administered eighty grains of sulph. quiniæ, by enemata. On the 24th of May I left her fairly convalescent. I was absent nine days, and on the day before my return, she had a slight relapse; of this she is now cured by twenty grains of quinine, by enema.

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*Remarks on Medical Organization.* By HENRY HARTSHORNE, M. D., of Philadelphia.

A close attention to the proceedings of the recent National Convention at Richmond, has given rise to some reflections, which may be expressed briefly.

It seems to have become a general opinion, that the precise mode of representation by which the national body has been aggregated, is a mere "provisional government," suited to the emergency of its first formation, but inappropriate to its maturity. Several different plans proposed for its modification in 1851 were reported upon this year, by a committee appointed for the purpose; and the subject was then referred to another committee, which submitted a series of definite propositions for amending the constitution. These will come up for adoption or rejection next year. In the meantime, it is of some importance that the profession generally should form correct conclusions on the subject, so as to influence the decision of the Association.

We have no intention of occupying space with a discussion of the whole subject; but stop merely to observe, that all such reforms must be gradual, and that the committee of 1852 acted wisely in preferring, at present, an alteration in the *ratio* of representation of Colleges and Hospitals, to their entire exclusion.

But a topic to which more particular attention may be directed, is that of permanent membership. The propositions submitted by the committee of this year, and adopted as a report by the convention, to be finally acted upon in 1853, embrace a provision for the abolition of all permanent membership; leaving to the Association an interrupted life only,—having no members except the delegates appointed for the particular meeting of each year. Now, to this alteration, there exist many and weighty objections. In the first place, with regard to the permanent members already



created, it would seem to be a measure of questionable justice and right, summarily to deprive them of a privilege guaranteed by an original and fundamental regulation of the constitution under which they served. But, waiving this, we may take the ground of an established precedent, and assert that the *onus probandi* rests with the proposers of the innovation, to justify it, if they can. *What harm* is done by the existence of permanent memberships? What good will be effected by their abolition? These are the questions at issue. If they be answered negatively, it must be an error to make the change.

Now, as the permanent members, not delegates, have no vote, it is impossible for them to do any harm, unless by the power of speech, in the meetings of the Association. And the fact of their having this power of speech, need only increase the care and responsibility of each of the bodies sending delegates, that those sent may not be such as to act as either incendiaries or time-killers at any future time. The Association has, moreover, of course, the power to expel members who have proved themselves really unworthy.

But, it has been hinted, that the institution is not republican; that it creates a privileged class; an aristocracy. Not so, by any means;—because these privileges are within the reach of all, in turn, and should be made to rotate amongst all, whose characters and attainments are worthy of a place in the great American fellowship. If there be *any* comparison, where members compete, as must be the case to some extent in every profession and in every class, surely "*οι ἀριστοι*" ought to be those upon whom the principal choice should fall. The plan of constituting every member of every county society, ipso facto, member of the National Association, and conferring upon each a diploma to that effect, would have this result simply; that it would add one more to the number of formal certificates whose real value is in inverse proportion to their abundance, and which carry but little force in their assertions. Every member of a County Medical Society is a member of the great body of American Physicians; but what would be added to the fact by its inscription upon parchment? Or why should this fact interfere with the existence of a special privilege, for a particular purpose, delegated each year to some, though open in turn to all?

But the real argument of importance against the abolition of permanent memberships, is in connection with the appointment of the Special Committees of the Association, upon scientific subjects. How it would narrow the selection and efficiency of these, to carry out the proposed change! It was, indeed, with judgment, proposed this year to extend the service of the Committees on Epidemics to five years, to allow of the accumulation and digestion of more valuable facts. How, then, could this be done, if those appointed delegates one year, cease with that year to be members of the Association, and may happen never to be appointed again? The most cursory observation of the requirements and difficulties connected with these committees, must show the immense advantage of leaving, for their selection, the wider range allowed by the present order of the constitution.

But it is in immediate connection with the business of these committees of observation, that we would beg leave, with great diffidence, to offer a few farther suggestions. Their germ was found in a report read at Richmond, this year, by Worthington Hooker, M. D., of Connecticut, as chairman of a committee on Epidemics. In this was expressed, very clearly, the disappointment of the committee in their attempts to obtain full materials for their report by correspondence. But one or two answers were obtained, if we recollect rightly, out of a large number of letters and circulars addressed to physicians through an extended district. It was hence inferred, that a deficiency or inapplicability exists in the method by which these materials were sought; and a wish was expressed, that some mode of organization might be adopted, by which medical observation could be made more available to the same ends.

It appears to the writer, that nothing more important could be suggested in connection with the business of the National Association. The objects of this great organization of the profession would seem to be chiefly of two kinds: 1. Medico-legislative or ethical,—and 2. Scientific. And, as all medical legislation looks to the application of science as its subject of control, we may consider the advancement, diffusion, and legitimate use of medical knowledge to be the great ends of all professional organization. To *accumulate* and to *concentrate medical observation*, systematically, throughout so large a country as ours, would be



one of the greatest benefits it could confer. It affords also excellent opportunities for accomplishing this. It has been attempted, by the appointment of yearly committees, for instance, upon Epidemics. But how has this plan succeeded? Besides Dr. Hooker's, numerous other reports have asserted, upon the same and other occasions, a large preponderance of disappointment. The question then remains, whether some other plan cannot be devised, by which stimulus, facility, and universality may be given to the collection of data concerning disease, and their centralization ensured for the benefit of the whole country.

For this, it needs only a methodical and determined commencement, originating in the central body, and ramifying throughout its local constituencies. In other departments of science, (meteorology for instance) all are familiar with the extent and success of plans for the accumulation of detailed facts and observations, by the issuing of blank forms and circulars, to be filled up in scattered localities, and all returned to a central generalizing functionary or board. In the Report of the Committee on Hygiene, of the American Medical Association, published in its transactions for 1851, a recommendation, somewhat similar to this, is given, in pursuance of a suggestion from the Smithsonian Institute; but still very limited in its bearing. This committee "recommend that the blank forms furnished by the Institute should be distributed by the State Medical Societies to their members, with an earnest request that they should make faithful returns of all deaths occurring in their practice. That the returns received by the State Societies should be tabulated and arranged upon a general and uniform plan by each society,—and the returns and tables prepared from them sent to the Smithsonian Institution, where their compilation and arrangement into a more comprehensive form could be effected by a committee of that body."

The great utility of such an arrangement, if carried out, is manifest; all that we object to it is, that it is *less* useful, and less practicable and certain than the interests of the profession and the community demand. Why should such returns be confined to the cases of *fatal* disease? These alone may be appropriate to the compilations of the Smithsonian Institute,—but, for the National Medical Association, it would seem desirable to obtain

more ample and instructive results. And, again, the mode of distribution of the forms of inquiry appears to us to be incomplete. They are to be furnished by the State Medical Societies to their members. Such forms should, rather, be furnished by every County Society to each of its members, with instructions to return them, with answers, to a committee of the Society. This committee, then, forwarding its report to the State Society's committee, a summary or compilation of returns from all the counties within its domain might be in turn handed to the Committee of the National Association. If such returns, as to the nature, amount and results of prevailing diseases, were thus concentrated at stated periods in the hands of a committee serving the national body for five years, most important data could no doubt be thus obtained, in a shape more capable of accurate generalization than any less extended and systematic research could give. It is probable, also, that no difficulty would exist in the way of obtaining full and regular answers to such definite questions as might be thus uniformly circulated. Method, the subdivision of labor, and the use of the printing press, make many things easy, which, without these aids, might seem unattainable. At any rate, the plan now in vogue having failed, to a considerable extent at least, it would seem a matter of evident expediency to give trial to any other that appears at all likely to succeed.

We will conclude, by merely hinting at the kind of questions, which might form the basis of a circular of inquiry, to be distributed at stated periods (quarterly, for example) by a committee of each county Society, to its members.

1. What diseases have been most prevalent under your observation during the past three months?
  2. Have any *peculiarities* attended the course of the prevalent maladies you have observed during that time?
  3. Has any peculiarity, of type or tendency, affected the course of *ordinary diseases* during that time, within your limits?
  4. Have any *new results of treatment* in disease come under your immediate notice during that period?
  5. What has been the number of deaths in your practice during the past three months; and from what cause in each case?
- If complete and universal returns were not at first obtained



to all these queries, we imagine that more at least would be produced than by the present plan; and the important effect would also follow from it, that practitioners would thus be stimulated and assisted in the task of recording constantly the main items of their observation, and of making them available for the common benefit of the profession and of humanity.

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*On Chloroform as an Emmenagogue.* By DAVID H. GIBSON,  
M. D., Fort Towson, Choctaw Nation.

Having nowhere seen, in the course of my professional reading, any allusions made to the use of chloroform, as an emmenagogue, I am induced to submit the following facts for publication, partly from a desire that relief may be afforded to the suffering, and partly from a sense of professional duty.

Cases I. II.—Occurring in the same person. In October last, Mrs. W——, having a violent headache, to obtain relief resorted to the inhalation of chloroform. Within an hour after the inhalation (which was but for a few seconds) she was flowing freely, and continued thus for *four days*. There was no irregularity of the function of menstruation in the succeeding month (Nov.), but another attack of headache supervening, she again had recourse to the chloroform, and in a half hour the menstrual secretion made its appearance, the discharge continuing for *five days*. In both instances, the chloroform was inhaled about ten days after the subsidence of her regular periods. Since the last inhalation, she has menstruated at her usual period. Mrs. W—— is slightly inclined to plethora, general health usually good, aged thirty-five years.

Case III.—In the absence of Mrs. W——, from home, her servant girl, having gotten hold of the chloroform, imitated Mrs. W.'s example. A like result was produced upon the girl, who menstruated for four days. The girl is very healthy and about thirty years of age. The inhalation was never renewed by her. In this case, the chloroform was inhaled two weeks prior to her usual period, at which time she again menstruated. Since then she has menstruated regularly.

Case IV.—Miss ——, aged 19—general health excellent—no deviation having ever taken place since her first menstrual period, was, during a visit to Mrs. ——, induced to inhale chloro-

form, through curiosity to experience the sensations produced by it. In a half hour the menstrual fluid made its appearance and the flow continued for four days. The inhalation in this instance was ten days antecedent to the regular period, with which it did not interfere. Mrs. W——, my informant in regard to the foregoing cases, is an intelligent and reliable lady.

Case V.—Came under my immediate observation. Was called to see Mrs. H——, found her suffering much from suppressed menstruation. To relieve urgent pain, ordered hot hip-bath, from which the patient experienced much relief. Waited three hours after the use of the bath, without recourse to any other means, having decided, as this was an opportune case, to exhibit the chloroform, which was done for *thirty* seconds. In *twenty* minutes after its administration, the patient was flowing freely and continued to do so for three days. Patient is of a weakly constitution, the result of much hardship. Age of the patient, about forty years. This case is the more remarkable from the fact that the patient has not menstruated for more than *eight* months.

The suppression was induced by causes not deemed necessary to relate at present. Prior to the suppression, she had been very regular for many years. Pregnancy has nothing to with the case, as the patient is not at this time, nor for many years past has she been in that condition.

I regret that I have not a greater number of cases to submit for the consideration of the profession. Being but a young practitioner, I am desirous that more experienced physicians should give the chloroform a trial, in order more fully, than my position will allow, to test its value as an emmenagogue; and diffident of my ability to account *correctly* for the “modus operandi” of the chloroform in the above cases, I shall without comment submit them to those who have better opportunities for investigation.

Before closing, however, I will present the following case. Miss——, aged 18 years, had an acute suppression of the menses, upon the first recurrence of the monthly period, subsequent to the age of puberty. Epileptic spasms quickly succeeded the suppression. Three years have elapsed since, and she is yet subject to these spasms, at longer or shorter intervals.



Within the last eight months, she was placed under my care. At one time, she will menstruate healthily—at another, there will not be the slightest appearance of menstruation—again, a leucorrhœa will take the place of the proper menstrual fluid. Sometimes, the leucorrhœal, as also the menstrual discharge, whichever it may be, will appear a week, sometimes two weeks antecedent to the regular period. Without entering into detail, as regards the treatment adopted in this case, I will submit the subjoined query. Upon the hypothesis, that the chloroform did act as an emmenagogue in the cases already related, how far would it comport with the safety of this epileptic patient to administer the chloroform? This question is predicated upon the belief, that, if the menstrual function were regularly performed, recovery from the epilepsy *might* take place. It is intended that the question shall refer more particularly to the epileptic condition, than to the irregularity of the menstrual function. I have no reason to suppose the existence of organic disease, either of the brain or uterus, in this patient.

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*A new instrument for cauterizing the Urethra.* By E. S. COOPER, M. D. Reported by L. C. Lane, M. D., of Peoria, Ill.

An instrument for cauterizing the urethra has been invented by Dr. Cooper of this place, which for facility of application and certainty of results is superior to all other means hitherto used, combined.

It consists of a copper catheter, with the end for half an inch a little smaller than the body, and perforated with several holes. This is introduced down to the stricture, and then filled with dilute nitric acid, which, acting on the copper, soon produces the nitrate, which coming in contact with the urethra through the holes, produces cauterization to the extent desired.

The strength of the solution and the length of time the instrument is permitted to remain, regulates the degree of cauterization completely. Dr. Cooper generally uses one third of nitric acid, and two of water, and permits the instrument to remain for one and a half minutes, though a much shorter time will often answer.

The shape of the instrument may be varied to suit the case; thus, when several strictures exist in the strait part of the urethra a

a strait catheter might be used, with holes at several places to correspond to their number and location.

Though great contrariety of opinion exists among medical men in regard to the degree of cauterization most valuable, this instrument commends itself alike to all; for whether it is believed that caustics should be applied boldly so as to cause the detachment of a slough, and thus physically enlarge the canal, or by a slighter application modify the action of the lining, the variations are easily made with it.

Peoria, (Ill.) April 15th, 1852.

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*On the Nutrition of Muscles during their Contraction.* By E. BROWN-SEQUARD, M. D., of Paris.

It is generally admitted that when a muscle is in a state of powerful contraction, circulation, and consequently nutrition, are nearly arrested in it.

The well-known fact, that we are only able to maintain a permanent contraction in any of our muscles for a short time only, has been explained by a loss of strength occurring, from the supposed insufficiency of their nutrition. I have frequently performed a very simple experiment which proves that the cause of the rapid diminution of the power of our will, in that case, does not exist in the muscles themselves.

The experiment referred to has sometimes been made on my legs, sometimes on my arms; and it was conducted as follows: I took a weight in one of my hands, and kept my forearm in a state of flexion, so as to form with my arm an angle of only 25 or 30 degrees. In that condition some muscles, and more particularly the biceps, were in a state of permanent contraction. My ability to maintain my fore-arm in such a position, lasted between eight and twelve minutes. When I found it was completely impossible for me to keep my fore-arm in that position, an assistant applied the wires of an electro-magnetic machine to my shoulder and my fore-arm, so as to excite the biceps, and some other muscles. When, without any effort of my will, my fore-arm was maintained, nearly in the same position, during more than ten or even fifteen minutes.

After one or two minutes of galvanization, I occasionally tried



again the action of the will, and I found that it was able to act anew.

If the explanation be true, that the muscle is not sufficiently nourished, and loses, in a great measure, its irritability during its contraction, and that for this reason the will becomes unable to maintain the contraction longer than a certain time, then the action of galvanism ought to be incapable of producing the contraction. If galvanism is able to act as it does, it is because the circulation of blood, the nutrition, and consequently the muscular irritability, have been very little diminished in the contracted muscles.

I have made another experiment, proving, also, that nutrition continues to take place in muscles during their contraction. If the communication established by the nerves, between the muscles of a mammal and its spinal marrow, is left entire; and if the circulation is completely stopped in that limb, by an amputation of the leg at the hip-joint, then it is found that the muscles of that leg, under the excitation of a powerful galvanic apparatus, lose their irritability after ten or fifteen minutes. On the contrary, if the same galvanic excitation is applied to the muscles of the other leg of the same animal, it is found that the irritability remains a long time without a marked diminution, and that it cannot be completely exhausted. It diminishes little by little, but never disappears entirely. Therefore, it is evident that nutrition may take place in muscles during powerful contraction.

Four distinct organs are active in the case of a voluntary muscular contraction. 1st, the brain, i. e. the organ of the will; 2dly, the spinal marrow; 3dly, certain nerves; 4thly, certain muscles. Which of these is the one which is deficient in the case of my first experiment? It is generally admitted that it is the muscular irritability. My experiments prove that it is not so. Consequently, it remains to know in which of the three other organs exists the deficiency. It appears to me that it is in the brain, because a great many experiments have demonstrated that the nerves and the spinal marrow, when put strongly in action, remain very active during a long time, provided that the circulation of blood continues in them.

In saying that the action of the brain is deficient, I do not mean the action of the whole brain, but that of the part of that

organ which is used in producing the contraction of these muscles, which are put in action.

From the preceding facts and reasonings I think I am justified in concluding :

1st. That it is the action of a part of the brain, and not the muscles, which is deficient, when our will is unable to maintain a permanent contraction in them.

2d. That circulation and nutrition are but little diminished in muscles strongly contracted.

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## CLINICAL REPORTS.

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*Clinic of Jefferson Medical College. Service of PROFESSOR DUNGLISON. February 21st, 1852.*

(Reported by J. AITKEN MEIGS, M. D.)

I am, to-day, gentlemen, enabled, through the kindness of Mr. Bates, of this city, who is present, to bring before you three cases illustrative of the nature and treatment of that form of defective or imperfect speech, known as "stammering." It is a subject not often, perhaps, examined in a clinic, yet it is unquestionably a morbid condition, and no better opportunity than the present could offer itself to explain to you its phenomena, and to investigate their causes. Mr. Bates, who has the three persons now before you under his care, with that candor and liberality which ought to characterize every one connected directly or indirectly with our noble avocation, had expressed, when I proposed the matter to him, on his calling to explain his views on impediments of speech to me, his entire willingness to permit me not only to exhibit some of his cases to you, but also to be the expounder of the methods he adopts for its rectification; and, after having done so, I shall endeavor to deduce for you the inferences at which I have arrived in regard to the *modus operandi* of his means and appliances, and to the great principles of management that flow therefrom.

To-day, such an examination and inquiry are especially appropriate, as I have been engaged, during the past week, in



another place, in expounding to you the physiology of phonation, and the modes in which the different vocal sounds are elicited in the glottis, and modified in the vocal tube. The vowels, as you well know, flow without obstacle, and consequently demand no consideration. They are simple modifications of the voice formed in the larynx, and are uninterrupted by the organs—as the tongue, lips, &c.—in their passage through the vocal tube; while the consonants require different, and, at times, complex and delicate movements of the tube; and, as their name imports, have to be sounded with the vowels.

Stammering is a temporary inability to enunciate, freely and distinctly, certain letters at the commencement of one or more of the syllables of a word. There is a broken or interrupted emission of the voice in the act of articulation, and a consequent disconnection of the sounds; and you will understand, that the consonants must afford great obstacles to the stammerer, as they do, also, to children learning to talk; inasmuch as they are necessarily more difficult of enunciation than the vowels, in consequence of being dependent upon an ever-varying disposition and arrangement of the parts composing the vocal tube. Especially is this the case with that class of consonants known as explosives—as *b, d, t, g, k*, &c. These letters have of themselves no sound, or are mutes. They do not admit of a continuous pronunciation like the *h, m, n, f, s, r, l*, but require to be associated with a vowel sound, before they can be enunciated.

Much difference of sentiment, you will find, has existed in regard to the essential cause of stammering; and views have occasionally been entertained, which are certainly far from tenable. By some of the best physiologists, all the varieties have been referred to a spasmodic closure of the glottis producing a sudden arrestation of the issuing column of air. That this is not always the cause of the affection, however, is shown, as we shall see, by the cases before us. The great fault lies in the spasmodic action of certain of the muscles concerned in the production of the voice, and in articulation. Often, as in Chorea or St. Vitus's dance, the slightest agitation serves to aggravate, in the most painful degree, the abnormal action. Indeed, the affection may not improperly be—as it has been—called, “Chorea” or “St. Vitus's dance” of the voice. The stammerer, on attempting to

enunciate a word or syllable, experiences difficulty or resistance at the commencement, and having but an imperfect control over the voluntary muscles of the vocal apparatus, he at once loses all confidence in his ability to produce the sound required, and there consequently results an irregular or spasmodic action of those muscles, which, for a longer or shorter period and determined by the degree of spasm, effectually prevents enunciation. In the case of the explosive consonants, the total interruption of the breath, and the badly regulated and insufficient volition, give occasion to the most painful spasmodic efforts on the part of the muscles more immediately concerned in articulation. This may be even extended to the whole body, which is thrown into a most distressing state of agitation to overcome the obstacle. At length the spasm ceases with the accomplishment of the act of expiration. It will now, therefore, be understood, why the complete interruption to expiration in the enunciation of the explosive consonants should be the most common phenomenon observed in stammerers. In the case, however, of the continuous consonants, an additional phenomenon occurs, in the sound being prolonged by spastic action for a much longer time than necessary.

Mr. Bates, who is an ingenious and liberal mechanic, has been studying, for some time, the nature and treatment of these distressing impediments to speech, and, as I remarked, has been kind enough to bring here several of the persons now under his care, that you may see me examine them, and hear me explain the mechanical contrivances which he employs to obviate them. He was himself, for a long time, a most intense sufferer, and, in consequence, had his attention earnestly and assiduously directed to the discovery of some means of relief. He has overcome the difficulty in himself, and has happily succeeded in enabling others to do the same. In the three cases now before you, and which are at present under his guidance, the spasm manifestly affects different muscles; and hence, although in each person the same amount of difficulty is perhaps experienced in enunciation, the difficulty may concern different sets of letters. Thus the resistance may more prominently affect the labials, dento-labials, linguo-dentals, linguo-palatals, or gutturals; and hence the value



of the physiological knowledge which teaches us the intimate mode of their production.

[The patients were now brought, *seriatim*, before the class, and made to read words and syllables commencing with different consonants, especially with those of the explosive class.]

In the *first case*, (R—— G——, æt. 26,) the utterance of the explosive letters is arrested, and accompanied by a singular and sudden spasmodic protrusion of the lower lip. In the attempt to articulate such words as *Boston, bunch, boat, pill, pant, Pope, &c.* an arrestation of the sounding breath occurs, accompanied by such protrusion, and the patient is thus rendered incapable of completing or perfecting the sound.

In the *second case* (R—— S——, æt. 34) the voice is arrested, and there is a sudden and energetic contraction of the lips. The voice cannot escape from the mouth, and the difficulty here is with those words which contain the dentals, as *Thomas, Doctor, stone, &c.*

In the *third case* (D—— D——, æt. 25) there is spasm of the muscles that close the glottis, so that on attempting to pronounce the gutturals in such words as *grey, goose, great, king, court, &c.*, the glottis is quickly and spasmodically closed, and the current of air prevented from issuing, except by jerks:

The great object to be accomplished in the treatment of these cases, is to overcome the proximate cause—the neurosis, or irregularity of innervation, indicated by the spastic condition of the muscular apparatus brought into play in the process of articulation. To effect this, it appeared to Mr. Bates, and it was confirmed by experiments instituted on himself whilst suffering under the infirmity, that if a plan could be imagined to prevent the total interruption of expiration, which occurs in these cases, the patient would feel confidence in his being able to elicit the particular sound, and in this manner the spasmodic efforts might be prevented. He accordingly invented several well devised instruments and arrangements, adapted to the different varieties of stammering; either by preventing the spasmodic action of the muscles concerned, or by restraining, by appropriate pressure, the irregular contractions of the muscles. For example, when the lower lip and chin, in the *first case*, were con-

fined by means of a simple broad bandage, like the one I show you, and pressure was thus exerted upon the spasmodically contracted *musculus orbicularis oris* of the lower lip, so as to prevent the protrusion of the lips, the letters, which were such stumbling blocks before, could be distinctly enunciated, and with a daily decreasing amount of hesitation.

For the *second case* Mr. Bates has contrived, as you here see, a small plate, fitting closely to the palate, and affording attachment to a light narrow tube, the posterior end of which opens into the mouth, looking towards the fauces, whilst the anterior projects between the lips. By this contrivance the current of air is made to be in part continuous, and the patient finds, to his surprise and delight, that he can produce the sound without any limitation other than his will.

The subject of the *third case* has been materially benefited, and is, indeed, in a fair way to be entirely cured of his unfortunate habit, by means of a neckerchief or cravat, in which is a little spring, pressing—as you observe—directly upon the projection of the thyroid cartilage, in such a manner as to relax the *rima glottidis*, by approximating the thyroid to the arytenoid cartilages; thus permitting the exit of air and preventing the spasmodic action of the muscles that close the glottis. The spring is so regulated, that the amount of pressure upon the thyroid cartilage can be increased or diminished, as occasion may require.

[The effects of these different forms of apparatus were exhibited on the stammerers before the class; and the action of each was clearly manifested.]

By such contrivances, which are simple, and adapted to the accomplishment of the object in view, Mr. Bates succeeds in effecting a great desideratum,—the *restoration of self-confidence*,—the want of which is a main obstacle to improvement in all such cases; for as soon as the patient becomes thoroughly and practically convinced that there is no difference between his vocal organs and those of his friends, whom he hears speak without difficulty or hesitation, he becomes inspired with confidence in himself, and his exertions are thenceforth the commencement of his restoration. Let it be, however, distinctly understood—and no one is more satisfied of this truth than Mr. Bates himself—that no appa-



ratus of this, or any kind, can, of itself, accomplish a cure. Nothing but a thorough system of practice and discipline, persevered in for a considerable period, can effect the work of restoration. The instrument merely infuses confidence. Withdraw it, and the case is left in the same condition as before. I can have no doubt, however, that with such a system, the stammerer may be amazingly benefited by the course pursued by Mr. Bates; but he must be thoroughly impressed with the fact, that time is one of the elements that must enter into his calculations, and that no plan can succeed without it.

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#### BIBLIOGRAPHICAL NOTICES.

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*Elements of Chemistry, including the Applications of the Science in the Arts.* BY THOMAS GRAHAM, F. R. S., Professor of Chemistry in University College, London; &c., &c. *Second American, from an entirely revised and greatly enlarged English Edition; with numerous wood engravings.* Edited, with notes, by ROBERT BRIDGES, M. D., Professor of Chemistry in the Philadelphia College of Pharmacy, &c., &c. Philadelphia, Blanchard & Lea, 1852. Part I. pp. 430. Royal 8vo.

The appearance of a second American edition of this standard work on Chemistry may be regarded as, of itself, a strong evidence of its appreciation by the cis-Atlantic student. In no department of science has greater progress been made, and in none have more important facts and principles been developed, within the last quarter of a century, than in Chemistry. Especially is this remark true in reference to its application to Medicine, and chiefly, we may add, to *practical* Medicine. The time has not long passed by when Chemistry was scarcely suffered to claim allegiance with the other branches of medical science. It was looked upon merely as one of the departments of Natural Philosophy, with the leading principles and laws of which, every ac-

complished and well educated physician was expected to be acquainted, very much as he would be supposed to understand the principles of Mechanics or the laws of Light; but with no definite idea of its applicability to the practical part of his profession.

Well do we remember the almost universally prevalent feeling on the part of medical students in relation to the chemical lectures. Nearly without exception, was a distaste evinced for that particular branch of their studies, from their regarding it as almost an arbitrary arrangement of the schools, to be compelled to spend their time in the acquisition of a knowledge, the practical bearing of which on their future profession, they could not understand. No doubt much of this indifference of the student towards the chemical lectures was fostered by the too abstract method, then so prevalent, in which the subject was presented to him by his teachers. He would witness, perchance, the repetition of some of the brilliant experiments of Faraday on electricity, or of Melloni or Brewster on light; or he might be dazzled by the splendid proofs of the agency of oxygen gas in combustion; and even admire the beauty of the changes produced in the various decompositions of the metallic salts; but he never could realize that Chemistry constituted an integral—*indispensable* portion of his medical studies. He had, probably, a little while ago, during his collegiate term, listened to just such a course of lectures on Chemistry; and had then, as now, considered it as naturally allied to the course of mathematics or of classics, quite as much as to medicine proper.

Now, however, the case is widely different. The teacher of Chemistry has it in his power to illustrate almost every step in his course of lectures, by appeals to medical facts of the most vital importance to the physician. The so-called imponderables—Heat, Light, Electricity and Magnetism, are now shown to be most intimately concerned both in the phenomena of life, and in the treatment of disease. The experiments on the supporting power of oxygen to combustion now merely serve to illustrate and enforce the indispensable importance of that element of the atmosphere to every living being, in each act of life. The various saline and metallic compounds, the study of which formerly proved so irksome to the student, are



now invested with an unexpected interest in his eyes, when they are exhibited to him as constituting most valuable agents in the treatment of disease, or as possessing poisonous or antidotal properties.

But it is chiefly in the department of Organic Chemistry that the greatest advances have been made, and the most important facts developed. It is this portion of the subject that more especially interests the medical man. Here the most important and useful contributions have been made to the science and practice of medicine. Before the splendid discoveries of the organic chemist, how little was known of the true pathology of disease, and how necessarily meagre and empirical were our therapeutics! Now, however, what a flood of light has been thrown upon diseases of the blood and urine, for example, by organic chemistry! Indeed, it is not too much to assert, that the practitioner of the present day is more indebted to the developments made in Chemistry, for correct views in pathology and therapeutics, than to any, if not to all, of the sister branches of medicine.

Professor Graham's "Elements" have been so long and so favorably known to the profession, as to require no additional commendation on our part. In the words of the American editor, "the copious selection of facts from all reliable sources, and their judicious arrangement, render it a safe guide for the beginner; while the clear exposition of the theoretical points, and frequent references to special treatises, make it a valuable assistant to the more advanced student."

The American publishers have, as yet, only issued Part I. of the work. This embraces the consideration of the laws of Heat and Light, together with a very full and complete exposition of the principles of nomenclature, and the fundamental doctrines of Chemistry under the heads of Combining Proportions, Atomic Theory, Doctrine of Volumes, Isomorphism, Isomerism, Constitution of Salts, Chemical Affinity and Polarity, (including under the latter head the subject of Voltaic Electricity,) together with the new subject of the Atomic Volume of Solids. On these principles and doctrines of the science the author is particularly clear and explicit; and he may with great advantage, as well as confidence, be consulted by the advanced student. We need hardly

mention the fact that to Mr. Graham is due the discovery of the "law of the diffusion of gases;" and that we are indebted to his investigations for the interesting facts concerning phosphoric acid and the phosphates. Next in order, the simple non-metallic bodies are treated of, together with the compounds which they form with each other. Then follows the consideration of the metallic elements, which are treated of in Part I., only as far as the metallic bases of the earths, inclusive. The remainder of metals, together with the whole subject of Organic Chemistry, is reserved for Part II., which the publisher promises to have ready during the present year; and which may probably claim our notice at a future time.

The present edition is considerably larger than the former one, many portions of the work having undergone revision; and the number of illustrations is nearly doubled. The additions made by the editor are very judicious and useful; and we can most confidently recommend the work as one of the most complete and useful treatises on Chemistry with which we are acquainted.

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*A System of Operative Surgery, based upon the Practice of Surgeons in the United States, and comprising a Bibliographical Index and Historical Record of many of their Operations for a period of two hundred years.* By HENRY H. SMITH, M. D., &c. &c. *Illustrated by numerous steel plates.* Part III. (8vo. pp. 251 and 19 plates.) Philadelphia, 1852; Lippincott, Grambo & Co.

We find in this handsome volume a very respectable continuation of Dr. Smith's Operative Surgery. The superior style of paper, type and illustration, that proved so attractive a feature of the preceding parts, will fully sustain the reputation of the work in the part just issued. Nor do the letter-press contents appear to be any less creditable to the zeal and industry of their enterprising compiler, than were those of the pioneer portions of his publication.

The twenty chapters and nineteen plates which constitute Part Third, are devoted to the operations practised on the neck and trunk. They comprise, therefore, many of the most important and interesting topics of the whole book. The first



plate (29th of the series,) is occupied with the surgical anatomy of the neck. This is followed in the next (plate 30th) with a representation of instruments employed upon the œsophagus and trachea, including Bond's œsophagial forceps, Goddard's stomach pump, Physick's stomach tube, Nathan Smith's œsophagial hook, and other means and appliances of this kind. Plates 31st and 32nd respectively present views of operations performed on the trachea, the œsophagus and the larynx. Plate 33d exhibits some of the operations practised on the neck; and in plate 34th are depicted the "appearance and position of some of the tumors seen about the neck."

In plate 35th we are glad to find delineated, among other instruments, the aneurism needles of three of our Philadelphia surgeons—viz. that of Dr. Parrish, known as the Philadelphia needle, (although sometimes attributed to Dr. Mott, because that gentleman employs and recommends it), that of Dr. Horner and that of Dr. Gibson. Each of these has done good service in its time, and has been proved to be well adapted to the purpose for which it was designed.

Plates 36th and 37th present, in some twelve figures, large and small, certain operations upon the upper and lower portions of the neck, among which are included the ligature of the different arteries of those regions, and their respective relational anatomy, the strangulation of a large goitre, as effected by Liston, and the excision of the clavicle, as performed by Dr. Warren. Operations practised on the chest are well displayed in plate 38th; so likewise, in the eight following, (from 39 to 47 inclusive, chiefly copied from Bernard and Huette,) are those practised on the abdomen. These eight exhibit the opening of hepatic abscess, the treatment of wounds of the abdomen and intestines, the anatomical characteristics and relations of hernia in its different forms and conditions, together with the remedial management thereof, the operation for removal or production of artificial anus, and lastly, the ligature of the iliac arteries. The 48th plate of the series, and concluding illustration of Part Third, is occupied, in four figures, with the external characteristics of tumors of the neck and back.

So much for the engravings. We have endeavored, as briefly as possible in the foregoing enumeration, to give some idea of

the nature and amount of illustration provided by the author, in connection with his two hundred and fifty-one pages of letter-press discussions and descriptions. Imperfect as the analytical sketch of these illustrations evidently is, we regret our inability to afford, in any shape whatever, a similar account of the accompanying text.

The bibliographical index, with which the part commences, exhibits, in its references to two hundred and twenty-four separate papers on the different operations and other topics, an extent of laborious research which entitles Dr. Smith to the thanks of every student of American practice. Still it is a matter of regret that, with the exception of the citations given in the incidental foot notes in the course of the work, we have the periodical literature only to refer to, and that the more accessible as well as more permanent materials collected in the comparatively small number extant of genuine American books on Surgery, have not been allowed a place at least as conspicuous as that of the more ephemeral contents of the journals of the day.

Chapter 1st is occupied with the surgical anatomy of the neck. In chapter 2d, which treats of the parotid gland and its extirpation, we are glad to note that due honor is awarded to the late Dr. Geo. M'Clellan for his share in the establishment of this operation, "having done," to use the author's language, "more than any surgeon in the United States to demonstrate its reasonable character."

Chapters 3d, 4th, 5th and 6th are devoted, in regular order of succession, to the consideration of operations on the larynx and trachea (including cauterisation of the larynx, scarification of the glottis for œdema, tracheotomy and laryngotomy); to operations on the pharynx and œsophagus; to ditto for relief of deformities of the neck, whether from torticollis or burns, and lastly, to the study and management of tumors of the neck. Then we have chapter 7th on aneurisms in general; chapter 8th on aneurism of the carotid arteries; chapter 9th on the ligature of the innominate and subclavian arteries; chapter 10th, operations upon the chest; chapter 11th, ditto on the mammary gland; chapter 12th, do. on the walls of the thorax; chapter 13th, do. upon the abdomen; and chapter 14th on wounds in the abdomen.



The remaining six chapters are pretty much occupied with the subject of hernia in its different forms, relations and results, anatomical and surgical; and with the ligature of the iliac arteries, and the removal of tumors of the back and spinal canal.

With this meagre inventory we are forced to close the present notice. Want of time unhappily prevents us from dwelling upon many matters of doctrine and detail and historical interest, that would be likely to arrest a reader's attention in the hurried perusal which alone we have been able, for the present, to afford to it. We still hope to have the opportunity, with the entire work before us, of expressing more at large, and perhaps with less indulgence, our impression of the manner in which the author has acquitted himself of some portions of his onerous task. His undertaking is certainly an arduous one, and as praiseworthy as it is difficult—one that merits all the encouragement that can honestly be given to it in its progress, and the success of which we confess ourselves unwilling to impede by any hasty words of ours. We bid him God-speed, therefore, in his labors, and take our leave of him once more with many earnest wishes for their speedy and prosperous completion.

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*Obstetrics: the Science and the Art.* By CHARLES D. MEIGS, M. D., Professor of Midwifery and Diseases of Women and Children in Jefferson Medical College, at Philadelphia, &c. &c. Second edition, revised. Philadelphia, Blanchard & Lea, 1852.

Professor Meigs has long since taken the first rank among original writers on American medicine. His works on obstetrics and kindred subjects have rapidly gone through frequent editions, and have been well received both abroad and at home. There are about them a freshness, vivacity, and energy of style and thought, which, though sometimes degenerating into oddities and whimsicalities, have certainly proved attractive. Some of the critics have, indeed, carped at his manner, as wanting in the severity and dignity which should belong to the subject; but we suppose these rapidly succeeding editions of his books are a sufficient evidence that he has pitched his key to suit the ears of those for whom he writes.

The "Obstetrics" is an enlargement of the "Philadelphia Practice of Midwifery," which had previously gone through two editions. The first edition of the present work was published in 1849. "Though a large one, it was soon exhausted;" and of the edition before us, just issued, the author states that he has "considerably augmented it as to the text, endeavored to improve it by recasting some parts, cancelling others, and by an earnest attention to improvements in the literary execution of the whole." Our opinion of the first edition having been already expressed at some length, we must content ourselves here with briefly commending the book to the American student, as one of the best manuals on the subject extant, and adapted to his requirements. It were superfluous to say, that there is no better living teacher of the physiology, mechanism, therapeutics and surgery of obstetrics, than Dr. Meigs; and his book contains all that is known on these points, most graphically described, and aptly illustrated from the records of an experience, we presume now second to none in the world.

That Dr. Meigs entertains peculiar or rather extreme views on the pathology of some diseases of women and children, is well known to all who are conversant with the medical literature of the day. These views have been reiterated through most of his books; they have been discussed at some length in our columns; and we feel called upon here merely to say that we remain among those who cannot subscribe to all his doctrines "upon the nature, seats, causes and treatment of puerperal or child-bed fever," particularly as to its non-contagiousness.

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*The Medical Student's Vade Mecum: a Compendium of Anatomy, Physiology, Chemistry, Materia Medica and Pharmacy, Surgery, Obstetrics, Practice of Medicine, Diseases of the Skin, Poisons, &c. &c.* By GEORGE MENDENHALL, M.D., Third edition. Philadelphia, Lindsay & Blakiston, 1852.

Of the third edition, lately issued, of this useful work, we have great pleasure in echoing the generally expressed favorable opinion of the medical press. It is, in every respect, well arranged and well executed, and deserves the popularity which has been established by "the rapid sale of the two former editions." As



regards the form of "question and answer," by which this work is characterised, our opinion has been that it is needlessly cumbersome, occupying space that might be better employed. We presume, however, from its having been retained here, through three editions, that it is not unacceptable to the class for whom it is intended.

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## THE MEDICAL EXAMINER.

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PHILADELPHIA, JULY, 1852.

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### EXPLANATION.

We have been concerned to learn, that some expressions, which occur in a communication from Dr. E. B. GARDETTE, of this city, published in our last number, have been the subject of misconstruction. A letter has been addressed to us by a gentleman from Baltimore, in which exception is taken to allusions indulged in by Dr. GARDETTE in the communication in question, inreference to hospitalities tendered him, on his visit to that city to attend the examination of the Dental College. That these allusions should have given offence to the gentleman who addresses us, we most sincerely regret. The language complained of, did not certainly strike us, on going through the press, as susceptible of an offensive interpretation. We must admit, however, that there are grounds for a call for explanation as to the allusions. We beg to express unreservedly our own regret that anything, appearing in the columns of this journal, should have pained the feelings of a highly respected correspondent from Baltimore. And we are authorized to add, on the part of Dr. Gardette, a no less unqualified expression of regret in the matter; and to disclaim, most pointedly in his behalf, any intention of attributing a sinister or unworthy design to the Faculty of the Dental College of Baltimore in their personal attentions to him, on his visit to that city.

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In a recent number, we announced the resignation of Dr. JOHN BELL from the chair of Theory and Practice of Medicine, in Ohio Medical College, and his proposed return to his former residence in Philadelphia. We have great pleasure in laying before our readers, the following resolutions, passed by the several institutions with which he was connected,

on the occasion of his leaving them, as evidence of the high esteem in which he was held by them.

*Ohio Medical College.*

*Resolved*, That this Board has received with regret the resignation of Professor John Bell, of the chair of Theory and Practice in the Medical College of Ohio:—with regret for the loss which the Institution will sustain in being deprived of his valuable services, and with still more regret that loss of health should be the cause of his resignation of a seat which he is so well qualified to fill with honor to himself and this Institution; and with usefulness to his pupils.

*Resolved*, That this Board tender to Professor Bell their thanks for his past services, and their best wishes for his speedy restoration to health, and to the enjoyment of happiness in the ability to continue his usefulness to the Profession, and to society in general.

*Cincinnati Medical Society.*

*Whereas*, We are informed of the resignation of the chair of Theory and Practice of Medicine in the Medical College of Ohio, by our distinguished friend and professional brother, Professor John Bell, and of his intended return to his former home, Philadelphia, therefore

*Resolved*, That his eminent literary and scientific attainments, and paractical abilities, give full evidence of his entire devotion to the profession:—that we sincerely regret the loss of so valuable a member of our Society, and that it may be long before the vacancy thus created will be filled by one so truly worthy of the appellation of *scholar, physician, gentlemen and brother*.

*Resolved*, That a committee of three be appointed to invite Professor Bell to deliver a parting address to the profession and citizens, at such time and place as may be agreed upon; and to present him these resolutions as a token of our high esteem and sincere regard.\*

*Cincinnati Medico-Chirurgical Society.*

*Whereas*, This Society has learned that Professor John Bell has resigned his place in the Medical College of Ohio, and is about to remove his residence again to Philadelphia, therefore be it

*Resolved*, That it is with profound regret that we have heard of his determination to withdraw from our city and profession.

*Resolved further*, That this Society, having a high appreciation of large scholarship, great erudition, high medical attainment, and ripe judgment, as of the greatest importance in elevating our common profession, and making it more influential and powerful in overcoming and destroying the various empirical systems of our city, and the West generally, part with our distinguished friend with still more difficulty and regret.

\* This lecture was not printed owing to the immediate departure of Dr. Bell from Cincinnati. We hope, however, that we may still have the pleasure of seeing it, or, still better, of hearing it.—Eds.



*Resolved further*, That as this Society freely endorses the code of ethics of the American Medical Association, as containing noble, wise, and lofty sentiment, and calculated in an eminent degree to elevate our profession, it feels more deeply the loss of Professor Bell, who, from his distinguished position and gentlemanly bearing, would have been able to give great moral force to its enforcement, and to inspire our profession with general respect and strict observance of it.

*Resolved*, That Professor Bell be invited to deliver a valedictory address to this Society before his departure.

*Resolved*, That the Secretary be, and is hereby instructed to transmit a copy of these resolutions to Professor Bell, and also to furnish a copy to the Western Lancet for publication.

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#### MEDICAL NEWS.

*Extract from an Address delivered by Dr. SAMUEL GRIMES, of Delphi, at New Albany, May 20th, 1852, before the Indiana Medical Convention.*

"Before we blame with asperity the wrong bias of a portion of the community towards medical heresies and quackery, we, of the profession, ought to have the consciousness that we are clearly distinguishable from the whole tribe of pretenders, by our superior attainments, not only in medicine, but in general literature and science, as well as by our greater readiness of resource at a time of difficulty and danger in the sick room. The mere fact of our holding a diploma in Latin, while others put up with one in English, and of our believing in one doctrine, while they are clamorous in favor of another, will give us no valid claim on the world for higher professional rank, and greater skill in *practice*. We must rest our claims on something more substantive and appreciable than this. We may dwell with allowable pride on the antiquity, and learning, and signal philanthropy of the great family, with Hippocrates at its head, to which we *belong*. But the pride of ancestry becomes ridiculous if it is not sustained by contemporary merit. To be received in the great brotherhood and decorated with the order of merit, we ought to be able to show that we underwent early preparation by a good academic or collegiate education; and early training afterwards, by elementary medical instruction, in the office of an experienced physician, before we were enrolled and became regular and faithful attendants in a Medical School of repute and competent organization.

"Some, perhaps many of us, will confess, with regret, that we have not been able to comply with these requisites, in all their particulars. But, surely, this does not imply that we should withhold our advice and

assistance, in order to remove the obstacles from the path of those coming after us, which embarrassed and impeded our own course. The times are more propitious to the zealous student now than they were in our early days; and, consequently, there is less excuse now than then for neglect, on the part of a parent, to procure for his son a good preparatory education, and on that of a physician to impart to his pupil suitable elementary instruction in medicine. Nor need any of us stand excused from the discharge of his duty to his pupil, if he received any such in his office, by the plea of his own defective attainments. However limited may be our reading and knowledge of medical literature, we can still point out to the young student a good book on each of the different branches of medicine; and early familiarize him with the qualities of drugs, and, to a certain extent, their pharmaceutical combinations—while we give him opportunities of ascertaining their therapeutical value. Though ourselves not learned in Climatology, we can, without difficulty put him in the way of observing and recording the characters of the successive seasons, and the modifications caused by the particular locality in which we reside. Though ignorant of Greek, thanks to the Sydenham Society, we can put into his hands a good translation of the admirable essay of Hippocrates on *Air, Water, and Places*, and read with our student the interesting and instructive volume on the climate of our own great valley, by Dr. Drake.

“So in Botany, even if we are not able ourselves to teach, we might, without much difficulty, set an example to our students of learning its elements, and exhort them to prosecute their inquiries in this direction, combining with them the study of vegetable physiology. Continually surrounded by the productions of nature, the country student has stronger incitements, and at the same time, greater facilities, to become acquainted with Natural History than the resident of a city.

“In pursuing the course here recommended, we shall discharge not only our duty to our students, but, also, to the several Medical Colleges to which they will resort after leaving us, for on the proper elementary instruction which these young men have received at our hands, will mainly depend their successful indoctrination in the higher principles and rational practice of Medicine by the professors in the schools. These gentlemen are continually embarrassed in their teachings by not knowing what foundation has been laid by the private teachers, or by those who ought to have been such; nor how far they may take for granted an acquaintance with elementary truths on the part of their youthful auditors. One professor, for fear of going beyond the comprehension of his class, contents himself with uttering the merest *common places*; another aims



to bring up his hearers to his own elevated standard. If it be said that the first is the most popular lecturer, does not the fact show the unpreparedness of the students for a course of instruction such as they ought to receive, and such as an able and conscientious professor ought to be able to give.

"The dignity, learning, elevated standing, and successful teachings of the Faculties of our Medical Colleges cannot be matters of indifference to us. They are, in one sense, our representative, in another our auxiliaries; and whatever effects their reputation must re-act on the entire body of the profession. We cannot, therefore, be supposed to look with indifference on the organization of these colleges, nor be expected to give any countenance to those with the entire competency of whose Faculties we are not fully satisfied. A new Medical School cannot, like a new store, or a new manufactory, or a new line of stages, be patronized, merely because it is new, and to encourage trade and reduce prices, by competition. Nor can personal regards nor sectional pride furnish an apology for our sending our students, or otherwise giving our countenance to a school, formed in haste out of incongruous materials, and as if in a spirit of mere business speculation. The medical profession, at large, must divide the responsibility with the Medical Schools for the attainment and fitness of the graduates sent out annually from these latter, as candidates for practice and public patronage. If we send to the Medical Colleges crude materials, it cannot be expected that they should be at once fashioned into shape and harmonious proportions. If we send to them ignorant minds, we have no right to suppose that these will be instructed and taught the principles of medical logic, and philosophy in the course of a few months.

"I repeat, that we have a large share of responsibility for the manner in which Medical education is conducted. We send our students to be educated, we influence them in their selection of the school to which they go. We, therefore, mistake greatly our position, if we think that we can assume the office of judges and critics of Medical Schools without our participating in their cares and difficulties, and giving our counsel for their protection and guidance, when they come up to what we believe to be a good standard of teaching. The best assistance we can give them, is to send them students properly prepared to avail themselves of the higher course of instruction which the professors are ready to go through with. The best counsel we can offer to the professors, is for them to abide by their own convictions of duty in keeping up an elevated standard of medical attainments for their students, and taking adequate time for imparting their knowledge to these latter. Not only are there

more branches of medicine to be taught now, but each branch is much richer in details, than formerly; and hence it is clear that more time is required to teach them than formerly was requisite.

"It may be asked, whether we are ourselves, either by education or subsequent reading, competent judges, in the premises, of what is necessary on the score of Medical Education? It may be that many of us are not, in this sense *competent*, but the very consciousness of deficiencies, and experience of what we have suffered in consequence may naturally impel us to seek reform, and create a desire that others coming after us should enjoy advantages of which we were deprived.

"We may, however, all of us, in this age of periodical literature and abundant publications, readily procure the desired information on many subjects of immediate interest. The calls on our time and attention in the cares of practice, and especially country practice, must always prevent our being regular and systematic readers: but if we are bent on turning all the odd hours to account, and have a good medical journal, and the best monograph at our elbow, we shall be able to keep up, to a respectable extent, with the progress of our science, at least so far as to appreciate its extent and chief bearings, and the improvements in practice. In this way we can do justice to ourselves and to our students, and confer, if need be, understandingly with our brethren, the members of the Faculty of Medical Colleges.

"In order to give effect to our wishes and intentions on all that relates to the improvement and elevation of our profession, we must act through our State Society *organization*. Indiana, in this respect, will not, I hope, be backward in rivaling her sister States—some of which, as Pennsylvania, New York and Tennessee, furnish, in their transactions, commendable examples for imitation. For this purpose it is all important that the County Societies should be well organized—as on their efficiency will depend the weight and character of the State Society. From each of them we may expect an account of the medical topography of the county, and of the diseases to which it is subject in successive seasons, together with those of an evidently endemic nature. Clearness and brevity should be consulted in these descriptions and histories, if it is desired to render them acceptable to hearers or readers, and creditable to their authors.

"I would recommend our Society to petition the State Legislature for an act requiring a registration of births, deaths, and marriages, so that we may be furnished with valuable *vital statistics*, and which will serve as a basis for many exceedingly interesting inquiries both of medical and general value.



"As far as lies in our power we shall, it is to be hoped, be, all of us, willing to give weight and efficiency to the recommendations of the National Medical Association, to which we are already indebted for an excellent Code of Ethics, and for various Reports on the Literature and Practice of Medicine, and the means of elevating the standard of Medical Education. If the State and County Societies act in unison with the National Association or *Congress*, the best results will follow, not only to our profession, but to the nation at large."

PRESENT TO THE ROYAL COLLEGE OF SURGEONS.—Mr. Abbott Lawrence, the United States Minister at London, having stated to the Lords Commissioners of her Majesty's Treasury that two cases have arrived from New York, addressed to him, and containing fossils sent by Dr. John C. Warren of Boston, United States, for presentation to the Royal College of Surgeons, London,—their Lordships have given directions to the proper authorities of the revenue to permit their free delivery for the purpose stated.—*London Lancet*.

OPPOSITION TO VACCINATION.—Such is either the unwillingness or carelessness of parents regarding the vaccination of their children, in Great Britain, that medical men are compelled to seek the interference of clergymen to induce parents to have their children vaccinated. The Irish especially are opposed to the operation.—*Ibid*.

THE CONCOURS FOR THE CHAIR OF HYGIENE, in the Faculty of Medicine of Paris, which we noticed in our Journal of March 6, has terminated with the appointment of M. Bouchardat, Pharmacien-en-Chef to the Hôtel Dieu, to the vacant professorship. We learn, also, that M. le Docteur Dupré has recently been elected to the Chair of Medicine in the Faculty of Medicine of Montpellier.—*Lond. Med. Times*.

PROGRESS OF EPIDEMICS.—The small-pox is spreading in Jamaica. In the parish of St. Anne, where it had only been prevalent two or three weeks, there were upwards of 4000 cases. The greatest destitution prevailed, and, in many instances, poor persons perished for the want of assistance and of medical care. In Kingston there are several cases. The cholera, which was lately reported to be exceedingly fatal at Bokhara, is now stated to have broken out in some places in Upper Silesia, a part of the Prussian kingdom, where great misery prevails. If this be really the case, and it should extend, as it will in all probability, the mortality will be dreadful, for the dearth and misery in many parts of Germany

have, of late, been on the most fearful scale, so that the bodies of dead dogs, in a semi-putrescent state, have been devoured by the starving inhabitants.—*Ibid.*

OBITUARY.—Dr. JOHN DALRYMPLE, the able microscopist and oculist, of London, died on the 2nd of May last.

A medical man in Breslau has been fined 40 thalers for having neglected to attend to a patient who sent for him. (!)

M. CHOMEL.—Politics even affect the peaceable science of Medicine. M. Chomel, the celebrated Parisian physician, having refused to take the oath to the President, required by the *quasi*-Constitution, has vacated his Professorship at the College of Medicine.

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## RECORD OF MEDICAL SCIENCE.

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### MATERIA MEDICA AND THERAPEUTICS.

*On the Basis of the Eau Medicinale D'Husson.* By THOMAS BUSH-ELL, Esq.—The subject which I am about to bring under the notice of the Pharmaceutical Society relates to the history of a very powerful article of the Materia Medica, the “EAU MEDICINALE D'HUSSON,” which, although now obsolete, is still frequently referred to as the form in which colchicum was first introduced into medical practice; and, I trust, I shall be excused for occupying the attention of the meeting for a few minutes in relating some circumstances contributing to establish the identity of the *Colchicum Autumnale* as the basis of that remedy which enjoyed so great a repute about half a century ago as a proprietary specific for the treatment of gout and rheumatism.

A few days since I made inquiry of one of the chief vendors of patent medicines, at the west end of the town, if they now sold the Eau Medicinale; I was informed that it had not been inquired for for many years, but that they had been accustomed, when it was in demand, to receive it from a foreign perfumer in Bond Street. I then called at this house, and was politely informed that they were the proprietors and vendors of the Eau Medicinale D'Husson, but that they had not sold any for years, and that it had gone quite out of date. About four or five years ago they sold three or four bottles. I requested to be allowed to examine the remedy, and to see in what way it was sold or made up. After a little time two or three bottles were discovered; they held about two drachms, and were simply labelled “Eau Medicinale D'Husson.” Printed wrappers, descriptive of its powers, were not at hand, and the parties seemed to have



quite forgotten the doses in which it was administered. They informed me that the price was now nine shillings and sixpence, but was originally twenty-two shillings. The bottles were short and squat, quite curiosities in their way, and I could not resist revolving in my mind the great alteration which time and circumstances had made in the reputation and consequent sale of this remedy, which at one time must have realized a splendid income to its proprietor.

The earliest medical notice I can find of the Eau Medicinale, is by Dr. Jones, in the year 1810, who had been on the continent in the years 1802 and 1803, with a gentleman who was a great sufferer from gout, and had then heard of the remedy of Husson, and which, on his return home in the year 1805, he was induced to make use of. He derived so much benefit from it, that from this time it made great advances as a remedy in England, and, to quote the words of the editor of the *Edinburgh Medical and Surgical Journal*, "noblemen and philosophers concurred in sounding its praises, if not in dancing hornpipes, in testimony of the new agility and flexibility of toe with which it had endowed them; and the President of the Royal Society, Sir Joseph Banks, who experienced the most extraordinary deliverance from his arch enemy, is said to have made it almost his pocket companion." It now became a great desideratum to ascertain its composition. Rhododendron, Chrysanthemum, Digitalis purpurea and lutea, Tobacco, Elaterium with opium, and many other plants, supposed to be analogous in their action to the magic water, were stated to form its basis.

Dr. Pereira states in his *Materia Medica* :

"The circumstances which have led to the extensive employment of colchicum in gout, are the following: About seventy years ago, M. Husson, a military officer in the service of the king of France, discovered, as he informs us, a plant possessed of extraordinary virtues in the cure of various diseases. From this plant he prepared a remedy called Eau Medicinale, which acquired great celebrity for abating the pain, and cutting short the paroxysm of gout. Various attempts were made to discover the nature of its active principle. In 1782, Messrs. Cadet and Parmentier declared that it contained no metallic or mineral substance, and that it was a vinous infusion of some bitter plant or plants. Alyon asserted that it was prepared with gratiola; Mr. Moore that it was a vinous infusion of white hellebore with laudanum; Mr. Want that it was a vinous infusion of colchicum. Although most writers have adopted Mr. Want's opinion, we should bear in mind that the proofs hitherto offered of its correctness, viz., analogy of effect, cannot be admitted to be conclusive, as is well shown by the fact that they have been advanced in favor of the identity of other medicines with the Eau Medicinale."

I should apologize for making so long an extract from Dr. Pereira's work, but as it contains a perfect epitome of all I have ever seen on the subject of the so-called gout specific, and as it leaves the question of the identity of its basis open to further inquiry, I hope that the circumstantial evidence which I now propose to lay before the Society, may somewhat clear up the point.

Mr. Want published his opinion in the *Medical and Physical Journal*,

vol. xxxii., in the year 1814; and it was two or three years after that time, at the commencement of my apprenticeship, I was directed to prepare a preparation of colchicum in rum (four ounces of the dried cormus to a pint.) I was residing close to Covent Garden Market, and frequently had conversations on plants, &c., with the late Mr. Grimley, the herbalist, to whom I applied for the colchicum. Mr. Grimley inquired of me if I knew how it was that the use of this remedy was discovered by Mr. Want? He then related the following circumstance:—Mr. Want had returned home from visiting patients, and met his wife on the stairs, and told her that her father had been seized with a violent attack of the gout. The nursemaid, who was standing by, stated that she once lived with a little French gentleman, who prepared a secret remedy for the cure of the gout, the Eau Medicinale; that he was accustomed to carry on his operations at the upper part of the house; and that there was a door on the staircase which he always kept locked, but that she had managed to obtain a little bit of one of the articles he made use of, and that she then had it by her locked up in her box. Mr. W. had not much difficulty in gaining a sight of this treasure. It was unknown to him, but he at once proceeded to Covent Garden Market, and applied to my informant, Mr. Grimley, to ascertain if he could recognize the portion of bulb. Mr. G. pronounced it colchicum; and, to Mr. Want's further inquiry, informed him, that the only person who ever applied to them for it was a little Frenchman, and that he was accustomed to order from a cwt. to a cwt. and a half more than once in the season; that he adopted great precaution to prevent their obtaining a knowledge of what he did with it and from whence he came; that he always made inquiry if they had it ready, paid the amount charged, then brought a porter, who carried it to a hackney-coach which he had in waiting, and which immediately drove off with its precious charge—for precious it was, only to be compared to a lump of the Californian or Australian mineral, as its preparation sold for its weight in gold (two drachms twenty-two shillings.) This relation, although occurring about three or four and thirty years ago, is quite fresh in my memory. I have related it to many, and about seven years ago I was in attendance on the late Earl Thanet, who suffered much from gout, and had a great objection to take colchicum. I happened to mention this little history, when he said, "You are quite right." I met that little Frenchman repeatedly years ago at the late Duke of Queensberry's, who was a martyr to gout, and to whom he used to administer the Eau Medicinale, he being the preparer of it. In the course of inquiry I have also found good reason to suppose that this little Frenchman was a well-known foreign perfumer, and has been dead upwards of twenty years, the founder of the house who now profess to be the proprietors of this once celebrated remedy.

I have stated that it was in the thirty-second volume of the *Medical and Physical Journal*, that Mr. Want first made public his discovery of the basis of the Eau Medicinale D'Husson, but I find on further reference that it was not in a medical but in a popular journal, the *Monthly*, that he had previously announced it. I should take up too



much of the time of the Society, if I entered into the controversies which took place at this period, with respect to the reliance which could be placed upon this beyond the numerous other drugs which had been as confidently declared to form its active ingredient, and it was the opinion of many that ere long it would be consigned to the tomb of the Capulets. I find, also, that a charge was made against Mr. Want, by an anonymous correspondent, that he had gained a knowledge of its basis by means somewhat similar to what I have now related, but which he denied, stating that he had obtained the first hint from Alexander, of Tralles, who recommended a remedy, "Hermodactylon," for the cure of gout, and with the effects of which the *Colchicum Autumnale* entirely corresponded; and that this plant had entered into many gout remedies that had become obsolete. All I am now desirous to say on this subject is, that the blue chamber being once penetrated, its mysteries might be readily compared with those which had preceded, but I believe the key was obtained in the manner I have related to the Society.

The present holders of the recipe for the *Eau Medicinale*, were so ignorant of its powers, as to inform me that two or three drops were a dose. I find that the usual dose was half a bottle, to be repeated, if the effect was not apparent, in four or six hours, but it was not uncommon for parties accustomed to its use, to take the whole two drachms, and repeated cases are on record of its powerful and dangerous effects; even life in some instances fell a sacrifice to its imprudent exhibition; and such was the experience also with the *Colchicum Autumnale* when first brought into practice, as it was considered to be a specific, and administered accordingly. This is not the place for entering into the therapeutical action of this valuable and potent plant, but its botanical and chemical characters are highly deserving the attention of the Members, and, combined with its Pharmaceutical preparations, might form matter for most interesting discussion.

There is a galenical compound in one of the French codexes called *Eau Colchique D'Husson*, prepared as follows:—Dry *Colchicum*, 60; Sherry, 125; twenty drops in a glass of *eau sucrée* in gout and rheumatism; and there is a remark that this will be observed to differ from the tincture or anti-gout drops of Want, which has been given as the *Eau Medicinale*. Mr. Want's first recommendation was four ounces of the fresh root sliced to half a pint of proof spirit.

Before I conclude, I must direct your attention to the observations of Dr. Maclagan, lately published in the *Monthly Journal of Medical Science*, Edinburgh, and from which your own valuable Journal of this month has an extract. I observe that Dr. Maclagan states that the flowers of *colchicum* are supposed to be the part of the plant used in the preparation of the *Eau Medicinale*.—*London Pharm. Jour.*, April 1852.

## PATHOLOGY AND PRACTICE OF MEDICINE.

ST. BARTHOLOMEW'S HOSPITAL.—*Aneurism by Anastomosis; Death by Chloroform.* (Under the care of Mr. LLOYD.)—We regret to apprise our readers that the use of chloroform has proved fatal in this case. It will be clearly shown by the subjoined facts that no blame whatever can attach to any one concerned, the more so as the patient had, on the previous occasion, inhaled chloroform during a considerable time without any ill effects. We refrain from any comments, as we shall have occasion to return to the case when the post-mortem examination shall have been made.

Thomas Hayward, a rather spare and weakly man, aged twenty three, was admitted on Thursday, the 29th of January, 1852, under the charge of Mr. Lloyd.

The patient applied to the hospital for an aneurism by anastomosis occupying the whole of the right ear, and also to a considerable extent the soft parts in front and behind that organ. On and behind the ear, the vascular growth was elevated so as to form a large tumor. The integuments of the ear, as well as the diseased mass, were of a deep purple color. In every part there was a strong pulsation, as well as a loud aneurismal murmur; the temperature, however, was much the same as that of the surrounding parts. Projecting from the meatus there was a polypus or large fungus, whence issued a copious purulent discharge, which was often tinged with blood. Pain in the head was complained of, but no unpleasant sensations had been felt in the tumor itself.

The disease had existed since the patient was four years of age; various remedial means had been employed at different times, but without benefit. The patient had been treated at some of the London hospitals, where setons were introduced into different parts of the tumor, and portions of it enclosed with silver wires, which were now and then twisted so as to tighten them; but these measures were of no avail, and at length the patient was discharged as incurable.

Mr. Lloyd, not deeming the cure hopeless, and having consulted with his colleagues, determined to attempt the obliteration of the tumor—first, by deligation of the principal arterial branches in direct communication with the diseased mass, and afterwards by pressure applied to different parts in succession. With this view, on the 14th of February, 1852, the patient was placed under the influence of chloroform, and with the assistance of Messrs. Wormald and Paget, Mr. Lloyd placed a ligature around the temporal artery, just as it passes over the zygoma; other ligatures were applied in such situations as it was considered would tend most to cut off the supply of blood from the part affected, and pressure afterwards used before and behind the ear. The operation lasted, as was expected, for a long time, and the patient was kept under the influence of chloroform for *half an hour or more*. From the effect of the anæsthetic agent he recovered quickly, and when visited half an hour afterwards, he was found lying comfortably in his bed, and on being asked how he was, he answered, "Very well," and smilingly added that he



was very thankful the operation had been performed, and hoped it could soon be repeated.

After this, everything went on favorably: the tumor became much diminished in size, and the pulsation much lessened. But on further examination, a large artery was found beating very strongly, between the mastoid process and the ramus of the jaw, pressure on which part completely arrested the pulsation throughout the diseased growth. On this vessel, therefore, Mr. Lloyd determined to place a ligature, and appointed the 17th of March for the purpose.

The patient was taken into the theatre of the hospital, placed on the operating-table, and chloroform administered as on the former occasion. The anæsthetic fluid now used was from the same bottle as had been employed before, and the apparatus was also the same as usual. The chloroform was administered by one of Mr. Lloyd's dressers, who well understood, and had long had experience in its use. A gentleman of great experience, who has been a long time at the hospital, and two years house-surgeon, was watching the patient, and marking the state of the pulse. Other gentlemen were also assisting.

In from five to ten minutes the usual effect was produced, the patient having previously struggled much. The operation was then commenced, but no sooner had Mr. Lloyd cut through the skin, than it was stated that the pulse had suddenly stopped.

The chloroform was at once removed, but in a few seconds the patient had ceased to breathe, and no pulsation could be felt in any of the arteries, or at the heart. Artificial respiration, as well as percussion and compression of the different parts of the body, were immediately employed, with energy; and after continuing the means for a short time, the circulation was observed to be returning, and the act of respiration was several times performed. The state of inanitation, however, speedily returned; but by the employment of the same means as before, with the use also of galvanism, the circulation and respiration were again restored. But the patient fell quickly into the same state as at first, and was again brought round by the same means. In a few moments, the patient relapsed for the third time, when one of Mr. Lloyd's colleagues, coming into the theatre, recommended that the external jugular vein, which on the right side was turgid, should be opened, that tracheotomy should be performed, and the lungs inflated. These means were accordingly had recourse to.

The patient was besides placed in a warm bath at the temperature of  $104^{\circ}$ , artificial respiration being kept up all the time, and friction employed. All, however, was of no avail, and it soon became evident that life was irrecoverably gone.

The resuscitating measures had been continued for more than an hour. Ammonia had been applied to the nostrils, but no attempt was made to introduce any stimuli into the stomach, as Mr. Lloyd feared any liquid placed in the mouth might pass into the larynx, and occasion instant suffocation.—*Lancet*, March 20th, 1852.

*The Mechanism of Bronchophony.* By W. H. WALSH, M.D., Professor of the Principles and Practice of Medicine, University College, London, &c.

§ I. The physical conditions in which morbid bronchophony is observable are :

1. Increased density of the pulmonary tissue either surrounding pervious bronchi, or forming a medium of communication between pervious bronchi and the spot at the thoracic surface examined,—whether that increase of density be caused by solid, semi-solid, or, perhaps, even liquid infiltration of the parenchyma.

2. Increased density of texture, produced by extraneous pressure.

3. Presence of any solid extra-pulmonary formation in such a situation as to form a connecting link between the surface examined and a bronchus of some calibre.

4. Increased width and hypertrophy of the substance of the bronchial tubes.

5. Diminished density of the lung, as in the rarefaction of vesicular emphysema.

The diseases referable to the first head are the *common* causes of bronchophony; but any hypothesis in explanation of the phenomenon must also apply to the other and comparatively *rare* causes.

§ II. Laennec regarded bronchophony as an essential dependence on increased density of the pulmonary texture, and supposed that it was simply produced by the greater facility with which comparatively dense homogeneous tissue—homogeneous from the exclusion of air—conducted the laryngeal vibrations to the surface.

This explanation is inadequate to meet all the circumstances of the case. In the first place bronchophony *may* exist, and this to an intense degree, over lung rarefied to such a degree as to give actually almost tympanitic resonance under percussion. Of this fact, (not generally known, or, at least, taught,) I have observed a certain number of examples, where *post-mortem* examination left no doubt of the absence of any textural change in the lung except emphysema. In the second place, as was, I believe, first mentioned in print by Skoda, the voice resounds sometimes with greater intensity, when ausculted over the chest than over the larynx itself. In the third place the pitch of the bronchophonic differs sometimes distinctly from that of the laryngeal voice. These facts, without disproving the correctness of Laennec's theory as far as it goes, show at least that it requires an addition of some kind to make it include all the varieties of the phenomenon.

§ III. Skoda, holding the general doctrine that the "varying conducting power of the healthy and diseased lung-substance cannot be taken as a basis of explanation of any of the phenomena of auscultation," specially opposes Laennec's views of the mechanism of bronchophony on the following grounds :

- (a) Bronchophony may in the course of a few minutes appear and disappear over hepatised lung, the other physical signs, especially the percussion sound, having undergone no change.
- (b) When vocal reso-



nance thus suddenly disappears, it may as suddenly be restored by making the patient cough or breathe deeply, so as to free the bronchi from fluid in the part of the organ ausculted. (c) In cases of pleuritic effusion, the resonance grows weaker and weaker in proportion as the fluid increases; now, as the lung grows more and more solid, the greater the quantity of pleural fluid compressing it, the reverse ought to be observed, were Laennec's doctrine of solidification and improved conducting capability well founded. (d) If a healthy and hepatised lung be removed from the body, and if, while one person speaks through a stethoscope placed in contact with the surface of each organ successively, a second listens through another stethoscope placed at an opposite point of that surface, the listener will find that more intense resonance reaches the ear through the healthy than the diseased lung.

What is the force of these objections? Let us examine them *seriatim*. (a and b) Since I first became acquainted with Skoda's statement concerning the appearance and disappearance of bronchophony in hepatisation, I have repeatedly endeavored to produce artificially the change he describes, but have invariably failed; I have questioned other physicians on the point, and find that they have not observed the alleged variability in the vocal resonance. I have, however, in one or two instances, noticed intermittent bronchophony occurring spontaneously without the patient having expectorated or even coughed,—a peculiarity evidently of different mechanism from that insisted upon by Skoda, and one to which I will by-and-by return. (c) The argument derived from the phenomena of pleuritic effusion, seems feeble, and, indeed, unsound. The interposition of a mass of fluid between the condensed lung and the surface alters the terms of the problem completely. It has been shown by Colladon and Sturm, that sonorous rays which reach the surface of water at a very acute angle do not pass into the air, but undergo reflection in the interior of the liquid. Now the angle at which the sonorous vibrations reach the fluid from the bronchi, and ultimately reach the outer surface of the pleural fluid, may very possibly often prove of the degree of acuteness fitted to prevent their passage into the air. At all events Skoda, by ignoring the new influences likely to be exercised by the fluid, renders his argument valueless.\* (d) The results I have obtained from some experiments on the conducting powers of hepatised tissue, do not agree with those announced by Skoda. First: It is, I admit, quite true, that tissue, called hepatised, may conduct the voice no better, or even less forcibly, than a similar thickness of healthy parenchyma; but it is equally true, that this is not a constant result. I have occasionally found hepatised lungs taken from the body, conduct the sound with extreme intensity. And these varying results may be obtained from different lungs, which the naked eye would judge to be in the *same* state physically, in regard of their shares of air, fluid, and semi-plastic substance: but it is evident, from

\* Let it not be forgotten, too, that in the interscapular region strong bronchophony is sometimes audible, even where fluid is accumulated in large quantity in the pleural sac.

the variations in result, that, acoustically, they are in *different* physical states; and that, therefore, such experiments as Skoda's are not to be trusted to. Specimens of parenchyma, apparently identical, are in reality widely different. There can, for example, be little doubt, that varying homogeneousness plays a more important part, than any observable so-called solidification, in regulating the conducting power of the lung. In the varying homogeneousness of different specimens may, in truth, lie the key to the difficulty; but even, if so, it is a key which cannot practically be used. Secondly: If, while one person speaks into a stethoscope, with its narrow end introduced into the trachea, a second listens over a part of the chest where hepatised lung lies beneath, (and where intense sniffling bronchopony existed during life), the listener will often be surprised at the singular and total absence of sound. Skoda, obliged to admit this fact, attempts to evade its force by supposing the vibrations to be interfered with by fluid in the bronchi. To this, I would reply, that I have satisfied myself of the total absence of such *post-mortem* resonance over pneumonic solidification, in a case where the bronchi, to the third and fourth divisions, were peculiarly free from fluid, and scarcely any spumous liquid infiltrated the parenchyma—which very same parenchyma, removed from the body, conducted the voice from one stethoscope through another with striking intensity. If we consider the main difference in the physical conditions of the parts, when an individual himself speaks, or when another speaks into his trachea after his death, an obvious explanation of the experimental failures to imitate the bronchophony of life suggests itself. In the dead body, in truth, the laryngeal, tracheal, and bronchial walls take no part in the production or conduction of the sound, which is propagated by their contained air alone; whereas, in life, the walls of those tubes obviously conduct the sonorous vibrations. But to admit this, would have been fatal to more than one of Skoda's hypotheses. Besides, hepatised and healthy lungs are not strictly comparable in and out of the body in regard of this matter; *within the body* the contact of a hepatised lung with the chest-wall is more perfect than that of a healthy one; and, admitting that the former is a worse conductor in regard of the condition of its substance, it may be a much better one through the closeness of its union (especially if adhesive) with the parietes. Here is a point which has been totally overlooked by the Viennese physician and his followers.

§ IV. I conceive, then, that, whether Laennec's doctrine be true or false, the arguments just reviewed fail to prove it unsound. Skoda himself naturally thinks otherwise; and, excluding the walls of the trachea and bronchi from all share in the *conduction* of the sonorous vibrations of the chordæ vocales,\* (an office assigned by him to the contained air of those tubes alone,) maintains that bronchophony is really pro-

\* Skoda, of course, admits, that the bronchial walls, in the spot where consonance takes place, intensify the consonating tones of the air within them by their own vibration; but this is a very different thing from their taking part in the production of bronchophony by conducting the laryngeal voice.



duced by the consonance of the air in the bronchial tubes with the laryngeal voice.

The hypothesis of consonance does not appear to me satisfactory; and it unquestionably fails to meet all the conditions of the problem. The reasons on which I ground this opinion are as follows:—(a) Air in any enclosed space does not consonate with every sound produced at its orifice, but only with the fundamental note of that space, and with certain others having a fixed harmonical relationship to that fundamental note,—with certain of its *concord*s, in short. This is easily ascertained, in a rough way, by running the gamut with the voice at the mouth of an empty water-bottle; one note only of the octave is at all markedly reinforced by consonance within the cavity,—one or two others, (according to the distance from the orifice at which the vocal sound is emitted, and the depth of the mass of air within the bottle,) very slightly increased in loudness. Now, on the contrary, when bronchophony exists, it is audible with *successive notes* of the octave, standing in no harmonical relationship to each other—absolute *discord*s, in short. These successive notes are to be found at the lowest part of each register, (whether bass, tenor, or soprano); but the force of bronchophony gradually decreases, not at harmonic intervals, but on *each successive note from below upwards*, until the resonance disappears altogether;\* and obviously the limitation of bronchophony to grave tones, as contra-distinguished to acute ones, has nothing to do with the principle of consonance, but depends upon the difference in the number of vibrations and breadth of the sonorous wave, when notes of these two classes are severally generated. (b) Bodies consonate only in unison, or in certain fixed harmony, with the original sound which throws them into vibration.† Now, the pitch of the bronchophonic voice varies irregularly from that of the laryngeal with which it co-exists. This difference of pitch is especially to be caught in cases of hepatisation, and is sometimes very striking in amount; the corresponding notes heard in the larynx and on the surfaces of the chest are then, very perceptibly, *discord*s. (c) Skoda's exclusion of the tracheal and bronchial walls from all participation in the transmission of the laryngeal voice is at variance both with theory and experiment, and cannot for a moment be acceded to. (d) In cases where the bronchophonic voice is very positively and notably louder than the laryngeal, it is difficult to believe, from the mere fact of the intensity of the sound, that the phenomenon can be due to consonance. For a consonating sound, as a rule, is vastly more feeble than the primitive tone eliciting it; and the nicest adjustment of the quantity of air in the consonating body (presuming this to be hollow) is required, in order to produce any serious increase in the amount of intensity. Let it, however, be granted, *argumenti gratia*, that chance

\*Curiously enough, the vocal *resonance* always ceases to be perceptible at a lower point of the scale, than the vocal *fremitus*: this is another difference to add to numerous others distinguishing the properties of the two phenomena.

†The unison-note alone is distinct to ordinary ears; the consonating harmonies are so faint, as to require the organ of a Costa for their detection.

may sometimes cause the column of air between the larynx and the seat of bronchophony to be of the appropriate length to produce a marked increase of sound—the doctrine of Skoda gains nothing by the concession. For, be it remembered, Skoda rejects conduction as an element of bronchophony—bronchophonic voice is, in his apprehension, consonating voice; consonating voice is, then, under the above circumstances, by admission, louder than the original voice. Now, here is an idea irreconcilable with observation; for, it does not appear, that (provided the original and consonating sounds be produced by bodies of the same class, as vibrating strings, hollow bodies, solid plates, etc.) the consonating sound is ever louder than the original tone.\* (e) If the excess of loudness of the bronchophonic over the laryngeal voice were from consonance, *vocal fremitus* (inasmuch as the walls of the consonating tubes must vibrate in the direct ratio of the vibration of their contained air) ought to rise and fall exactly as *vocal resonance*. Now, in point of fact, these two phenomena do not invariably maintain any direct relationship to each other; one may gain, while the other loses, in intensity.

These objections appear to me conclusive against the pure doctrine of consonance, as taught by Skoda, while they show that, if consonance plays any part in the production of bronchophony, it must be a subsidiary, rare, and accidental one. And even this concession is rather made on the ground, that the occurrence of consonance within the chest is not, *a priori*, impossible, than in deference to the arguments actually adduced in its favor.

§ V. From the discussion into which I have now entered, it would follow that the mechanism of bronchophony is probably complex, and certainly not, as the attempt is commonly made to prove it, invariably one and the same.

There are four points which appear especially worthy of consideration: the conduction of laryngeal voice; its possible increase of intensity within the chest; the distance at which that increase of intensity (if real) occurs from the part of the thoracic surface examined; and the relationship of pitch of the laryngeal and bronchophonic voices.

1st. In regard of conduction of laryngeal sound, theory would support the inference, that as the human voice is best propagated in air, the more the lungs were rarified, the higher would their conducting power become; and, in accordance with this, it is certain that intense bronchophony is sometimes heard over highly emphysematous tissue. However, on the other hand, as the tracheal and bronchial walls themselves vibrate

\* The case may be very different where vibrations are communicated from a body of one physical constitution to a consonating body of another class. Thus, where a tube takes up the vibrations of a solid disc, the consonating note of the tube may (by managing carefully the length of the vibrating column of air in the interior) be made incomparably more powerful than the original tone; the quality of such notes is exquisitely pure and full; and, long ago, Savart suggested the construction of a musical instrument on this principle, which, it seemed probable, would exceed in melody and power any of those in use. Civilised musicians have not profited by the philosopher's suggestion; yet the savages of some of the Pacific islands, curiously enough, have hit upon rude contrivances illustrating the principle.



during speaking, any really solid material directly connecting a large bronchus with the surface of the chest, must conduct those vibrations forcibly; and, in accordance with this, we find that wherever solid fibrous structure is seated in the manner supposed, bronchophony of the most intense character is audible. But if the union of the solid material with the chest-wall be imperfect, if there be any interruption at the planes of union of the conducting materials, the acoustic conditions are completely changed, inasmuch as interruption at the union of media of different densities most deeply impairs the conducting faculty of the series. Here is one clue to the differences in vocal resonance, observed in cases where the physical conditions appear, on superficial view, identical; hence, too, we have no fair reason to expect that, in all samples of the variable semi-solid states comprised under the title of "hepatization," conducting power should be affected in an uniform manner, and experiment shows that it actually is not. Experiment, in truth, alone can teach, in each instance, what the force of conduction really is in the various complicated conditions of physical change in the lungs.

2ndly. It is indubitable, that the bronchophonic voice is sometimes louder than that transmitted through the stethoscope directly from the larynx. The extreme rarity of this occurrence does not affect its reality; and hence, some explanation must be found for the increase of intensity within the thorax. We have seen the objections to Skoda's hypothesis of consonance. Now, there is another way in which the intensity of sounds may be augmented at a greater or less distance from their place of production—by reflection, and by reflections brought to a focus, or *echo*. But are the conditions of reflection fulfilled in the hepatized lung? It would appear so; for the tubes along which the voice is transmitted from the larynx are surrounded by semi-solid material, proper (when compared with healthy tissue) to reflect and concentrate the sound; while the air-cells and minute bronchi are closed to a variable distance, and prevent its divergence. The tubes resemble so many speaking-trumpets, and, just as in these instruments, the augmentation of sound is produced by reflection from their quivering walls; as this reflection tends to propagate vibrations (otherwise divergent) in the same direction, increased intensity of sound must be the result. And, further, if the reflected vibrations chance to be brought to a focus within a large tube, then *echo* will occur; and, as under ordinary circumstances, the echo may be materially louder than the original sound. But, it may be inquired, how, upon this theory, is the temporary disappearance of bronchophony, already referred to as a possible, though very rare occurrence, explicable? It is conceivable, that where such suspension occurs, this may sometimes depend on the deadening influence of accumulated fluid in the tubes.\* It may also be sometimes traced to accidental pressure of a main tube by some extraneous body, which, from movements of the trunk, the act of coughing, &c., presses much or little, or not at all,

\*The effect of carpeting or woollen cloth of any kind, in deadening the sound of music in an apartment is well known. The intermixture of air and solid fibres in the carpets, through which the sound has to pass, deadens the echo

upon that tube.\* It seems extremely probable that, under the latter circumstances, bronchophony would, (I have not, however, verified this conjecture,) be heard at some point of the chest nearer the bifurcation of the trachea. Again, it is possible that certain changes of posture, altering the relationship of the reflecting surfaces, might interfere with the production of echo, by preventing the reflected sounds from coming to their usual focus. Besides, the position of the auscultator in respect of the focal point, might prevent him from hearing an echo really existing.† The force of the echo will also rise, the smoother the bronchial walls, and the larger the tubes in which it occurs. And numerous other circumstances may be conceived, but scarcely proved, to affect the phenomenon. Among these, the composition of the gases within the bronchi may, for aught that is known, hold an important place; hydrogen has been proved to deaden sound greatly; the effect of carbonic acid, mixed with other gases and aqueous vapor can only be ascertained from experiment.

Thirdly. As concerns the distance from the point of auscultation at which the reinforcement of sound within the thorax occurs: the further away, the less of the resonance will reach the surface; the amount, however, will be modified by the conducting property of the interposed media.

A little consideration will show that these three conditions of bronchophony—conduction of laryngeal sound, increased intensity of this within the thorax, and proximity of site of the increase—may or may not be directly as each other; one may be in a state favorable to, the rest unfavorable to, the formation of bronchophony. Hence the variable state of the sign in different cases of the same disease; and hence an additional clue, for example, to the inconstancy of bronchophony in pleuritic effusion.

Fourthly. The relationship of pitch of the bronchophonic and laryngeal voices seems the most difficult part of the subject—difficult, at least, in those cases where a distinct difference can be detected, in the pitch of the two. The consideration of this point is reserved for another occasion.—*London Med. Times.*

between the ceiling and floor, by which the original sound is swelled."—Herschel, art, "Sound," *Encyc. Metrop.* Aerated mucus and sanguineous serum in the bronchi would have the same effect on vocal echo in those tubes, as the carpeting under the circumstances referred to above.

\* This cause is suggested from actual observation.

† The existing theory of echoes generally is inadequate to explain many of their phenomena. There is (or was) a ruined fortress near Louvain well illustrating this. Here, if a person sings, he only hears his own voice, without any repetition; those who stand at some distance hear the echo, but not the voice,—and they hear the echo with surprising variations,—sometimes louder, sometimes softer,—now near, now distant.—*Burrowes's Cyclop., Art. "Acoustics."*



*London Hospital.—Abscess of the Lung following Pneumonia; Death; Autopsy.* (Under the care of Dr. PEREIRA.)—The formation of abscess in the lung as a consequence of pneumonia is considered a very rare occurrence; but it is probably still more rare to find a circumscribed accumulation of pus in the pulmonary tissue taking on a semi-gangrenous character. Such events seem in some degree to exclude each other; for the vigorous exudation indispensable for hemming in purulent matter can hardly be contemporaneous with sphacelus of the lung. However this may be, it is plain that in constitutions brought to a state of great debility by excesses, the most unusual pathological phenomena will spring up; and these being at all times instructive, we shall just, from the notes of Mr. Whitby, relate the following case:

John A. T—, aged forty, formerly a solicitor, married, and the father of four children, was admitted, January 18, 1852, under the care of Dr. Pereira. The patient is of a dark and sallow complexion, thin, weak, and exhibiting all the signs of a deteriorated constitution; his habits have been very intemperate, the chief beverage he indulged in being spirits. His health had, however, been tolerably good, until about five months before admission, when he was seized with pain in the right side of the chest, increased by a full inspiration; his habitual cough became very troublesome, the expectoration profuse, and of an offensive odor, pretty similar to that which was noticed on his admission. No medical aid was sought until three weeks since, when rigors had set in; which the patient tried to conquer by large potations of gin-and-water. The pain in the right side of the chest was then somewhat relieved by blisters, &c., but he was soon obliged to apply for admission into the hospital.

When first seen, the patient complained of cough and abundant expectoration; his voice was hoarse, the breath very short, the countenance anxious and sallow, the bowels relaxed, but the appetite still good. The tongue was furred and moist, with an unpleasant taste in the mouth; the pulse rapid and small, and the urine abundant, but very ammoniacal.

On examination of the chest, dulness was found over the right sub-clavicular and mammary regions, the corresponding parts on the left side being resonant: respiratory murmur, indistinct over a great portion of the right side of the chest, both anteriorly and posteriorly, whilst it was puerile on the left. To the right of the central part of the sternum a peculiar tremulous sound is heard, which has much resemblance with the pulsation of a large vessel through a cavity containing fluid. This sound was not, however, constantly noticed, nor could it be excited by any particular position of the body. The patient was aware of this peculiar bruit; he could himself hear it, and compared it to the noise made by water bubbling from the lid of a tea-kettle. The action of the heart was accelerated, but no other irregularity was detected. As to the voice sounds, pectoriloquy was heard at the base of the right scapula. When inquiring about the patient's sensations, it was found that the right side of the chest was painful, and that the cough was

extremely troublesome when he lay on his left side, the easiest posture being the slightly raised dorsal decubitus.

Dr. Pereira ordered bark and ammonia to be taken three times a-day. The symptoms continued pretty well unaltered for the next few days, during which the patient took, first chlorate of potash, and subsequently small doses of sulphate of quinine and morphia. The sputa remained of the same nature, the perspirations at night became very profuse, and the relaxation of the bowels could hardly be restrained by logwood and chalk. Thus the patient progressed for about three weeks, the amphoric murmur on respiration being audible over the right side of the chest, and the heart's action being accompanied by a peculiar gurgling and undulatory sound.

From the physical signs, and the nature of the symptoms which had existed previous to the patient's admission, it was assumed that he had suffered from pleuro-pneumonia, which had resulted in abscess of the lung, with adhesion of its walls to the parietes of the chest, besides effusion into the cavity of the pleura. Thus was a tremulous sound produced with each impulse of the heart, resembling the vibration produced by tapping a phial containing air and fluid. Twenty-six days after admission, the patient died suddenly, after expectorating a large quantity of very offensive matter.

*Post-mortem Examination.*—On viewing the chest, before proceeding to the inspection of the thoracic viscera, it was observed that the right side was larger than the left, and presented a rounded appearance, with the filling up of the intercostal spaces. The thorax, when opened, was found completely filled with pus of a very offensive odor. A large abscess was situated in the right lung, the outer wall of the cavity being adherent to the pleura costalis; and running in front of the lung in a transverse direction, was a broad sinus of communication, extending to the middle mediastinum, which region might be considered as the internal boundary of the abscess. The substance of the lung contained a large quantity of mucus, mixed with air and pus, as well as small tubercles and pieces of lymph. The bronchial tubes were filled with matter similar to that contained in the abscess, and the lung was adherent, over its whole surface, to the parietes of the chest. The left organ was perfectly healthy, but the cavity of the pleura contained about five ounces of serum. The heart was pale and of lax fibre, but otherwise in a normal condition.

It is clear that certain changes in the walls of the above described abscess, or a decomposition of its secretion, must have given rise to the fœtor so characteristic of gangrene. The latter did not, however, exist in the manner usually seen, namely, a crumbling down of the pulmonary tissue, and a dark livid color of parts. A circumstance which is finally worthy of record is, that the fœtor had existed from the commencement of the purulent secretion.—*London Lancet.*

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*A Case of Hydrophobia in the South of France.*—M. Guyon, of Valence, in the South of France, has communicated to the Société d'Emulation of Paris, the report of a case of hydrophobia, drawn up



in a very graphic manner, and presenting some points of interest. The patient was a nailer, about thirty years of age, who was bitten by a little dog, which he had never seen before, and which was quarrelling with his own. No signs of disease were noticed in the strange dog; it was seen for several hours afterwards to play about, gnaw bones, &c., and was subsequently lost sight of.

The medical man who was called in wished to cauterize the three small wounds with the hot iron, but the patient refused, and his surgeon readily yielded, as the dog was supposed to be healthy. Forty days (the period vulgarly believed necessary for incubation) passed away, and the patient now exclaimed that he had been more afraid than hurt, for he had felt uneasy in his mind ever since the accident. About a fortnight after this, the man began to give signs of peevishness, and became quarrelsome and abusive. He soon complained of pain in the vicinity of the pharynx, difficulty of swallowing came on, and the fits of dyspnœa, when water was placed in his mouth, were dreadful.

The surgeon in attendance noticed that the papillæ at the base of the tongue were much enlarged, that the epiglottis became quite erect, and resembled in shape and color a small cherry. Every time that he succeeded in lowering it, the respiration became easier. There was no horror of water, but a difficulty of swallowing it, the fauces, larynx and pharynx being highly injected. The spasmodic fits succeeded each other with fearful rapidity, the cellular tissue of the neck and chest became emphysematous, and the patient died asphyxiated the day after the attack.

No post-mortem examination was allowed, but the larynx and posterior portion of the tongue were taken out; they did not present any distinct alterations. M. Guyon appends to the case the following remarks:

1st. It is clear, judging from the case just related, that a dog can communicate the disease whilst the latter is yet latent; and if it be the salivary secretion which conveys the virus, it does not seem necessary that the animal have a great abundance of it, and foam at the mouth, for the fluid to be contagious as is vulgarly supposed. 2nd. It has been imagined that fear and apprehension have much influence on the development of hydrophobia; but this case shows that the disease may manifest itself more than a fortnight *after* every sort of apprehension has passed away. 3rd. The peculiar rising and stiffening of the epiglottis noticed in this case, has not been observed before, and would sufficiently explain the laryngeal spasms, which eventually brought on asphyxia. The phenomena of hydrophobia might thus be accounted for, without having recourse to a lesion of the nervous centres, which lesion has, in fact, never been demonstrated by autopsy. 4th. The emphysema of the neck and chest was probably due to the rupture of some pulmonary vesicles during the violent spasms which the patient had experienced. This circumstance shows very clearly that there must be direct communications between the general subcutaneous cellular tissue and the sub-mucous areolar texture of the lungs. 5th. The hypertrophied papillæ at the base of the tongue, which were seen very early in the course of

the disease, would explain the sense of pharyngeal constriction of which the patient complained at the very outset. Thus it would finally appear, that it is principally in the pharynx and larynx that hydrophobia becomes localised, although it is likely that the disease arises from a virulent infection of the whole organism.—*Ibid.*

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*Remarks on the Microscopic Characters of the Urine in Bright's Disease of the Kidney.* By JOHN D. MACDONALD, ESQ., R. N.

Having studied, from time to time, the microscopic characters of the urine in Bright's disease of the kidney, and compared the appearances there presented with those of a section, or torn-up portion of the diseased gland itself, when the occasion offered, several objects of interest were frequently to be observed, either little known or not at all described; and, as they illustrate and support the pathological views of the malady in question, so ably put forward by Dr. George Johnson, they may be worthy of notice.

The presence of lymph, or pus, is supposed to afford the only unequivocal evidence of inflammatory action, and this seems to be borne out in renal disease. Mechanical obstruction and passive congestion are the first effects of excessive distension of the uriniferous tubules, by whatever cause induced, and in the great majority of cases the antecedents of any active state which the vessels may take on. The condition of the kidney in these respects will be clearly indicated by the presence or absence of fibrinous casts of the tubules in the urine; but this part of the subject must be dismissed, as the remarks about to be offered chiefly refer to certain changes which are incident to the secreting cells of the kidney.

Admitting the faithfulness of the exposition given by Mr. Bowman, of the minute anatomy of the kidney, as demonstrated at any moment, a vindication of it may be called for, the pathology of the granular kidney seems to be simply this—an error of secretion, depending most probably upon the faulty character of the plasma from which it is derived, is attended with a gradually increasing deposition of oily matter within the cells, which are the principal agents in the process. The circumstance gives rise to an over-distension of the tubules, (more especially between the meshes of the matrix, where there is least resistance,) and thus, by mechanical pressure, the return of blood through the venous plexus which surrounds them is obstructed, the Malpighian tufts become congested, and may be ultimately ruptured; so that the presence of albumen in one instance, and actual blood in the other, may be detected in the urine. In some cases, the hæmorrhage which occurs in this way is truly alarming, and the more immediate cause of a fatal issue. Notwithstanding the close analogies existing between the liver and kidney, with respect to structure, function, and the diseases to which they are subject, there is this much to be said, that the kidney does not admit of congestion with impunity, while the liver may be gorged to a considerable extent, and yet suffer comparatively little disorder of function. There may be often some difficulty in determining the fatty nature of



the contents of the epithelial cells of the kidney, when discharged with the urine, more especially if little is understood about the changes which they are liable to undergo, in obedience to common laws. The most usual metamorphoses of these cells exhibit eight phases. In the first, distinct oil globules are to be seen within them, varying in number and size, and giving a more or less rounded figure to the cells themselves, which become enlarged in a proportionate degree. 2dly. The oil globules may run together, and so completely fill the cells, that (with the exception of the nuclei, which are generally visible,) they appear like starch granules, or little masses of oil, refracting the light powerfully. 3dly. The fat gradually assumes a more concrete form, loses much of its brilliancy, and presents a mealy surface, any remaining globules becoming angular and irregular in form. 4thly. This concrete mass accumulates around the nucleus, and a clear space begins to appear between it and the cell-wall on the opposite side, which may be partly due to the imbibition of water. 5thly. An analytical change takes place in the fatty matter, which separates into its two constituents, the more fluid part exuding through the cell wall, while the more solid presents a crescentic appearance, the nucleus occupying its centre. 6thly. The cell itself breaks down at its weakest part, and the crescentic mass exhibits a well defined convex border, formed by the remaining portion of the cell, and an uneven concavity where the cell is defective, and the concrete fat is exposed. 7thly. The little crescent continues to extend itself, the outline becomes better defined, and a fusiform figure is ultimately assumed, the nucleus still occupying the centre of the mass, having undergone no apparent change; and 8thly. The pointed extremities of this little fusiform body are drawn out into filamentous processes, so that when several of these altered epithelial particles unite end to end, as often happens, one is reminded of the mode of development of the white fibrous tissue; and on the whole they present such a close resemblance to cancer cells, that the history of their formation is of some importance, to be borne in mind with reference to diagnosis, especially if blood globules be also present with them; for it has been asserted, that if fusiform cells and blood discs are found to co-exist in the urine, certain indication is afforded of cancerous disease, affecting either the kidney or the bladder.

Now, many persons, keeping this doctrine in view, may arrive at once at a conclusion, without waiting to draw the distinction between an actual cell and a cell-like body. Forms very similar to those above described, abound in the spleen, with blood discs seemingly as nuclei, and differing altogether from the muscular fibre cells (so called) of the tubercular sheaths.—*Lond. Med. Times and Gazette.*

## SURGERY.

## CHARING-CROSS HOSPITAL.

*Convulsive Movements of Stump; Removal of the Nervous Bulbs; Amputation at the Shoulder-joint; Persistence of the Spasmodic Jerking.* Under the care of Mr. HANCOCK.

When the considerable number of amputations which are performed in public and private practice is considered, it might in some degree appear surprising that so few cases of painful stump come before the profession. What, in fact, becomes of the divided extremities of the nerves, in the great majority of amputations? They must certainly experience some compression, by the formation of the cicatrix, and yet the greater number of stumps are painless, and very useful; whilst it is to that very compression and irritation that the rare cases of pain and convulsive movements are attributed. The more or less bulbous state of the extremity of the divided nerves has no doubt some influence on the development of unpleasant symptoms; but it must be recollected that nerves are almost always found bulbous in perfectly healthy stumps. Must we seek the explanation of the abnormal state of parts alluded to, in the patient's temperament; or in the peculiar manner in which the bulbs may be incorporated with the bone and muscles; or in the size of the bulbous extremities? It is perhaps to a combination of these three causes that the distressing symptoms which have been recorded may be ascribed; but it may be presumed that the first of these causes—viz: the patient's peculiarity of temperament—has the greatest influence on the phenomena, especially where, besides pain, convulsive movements of the stump are manifested.

Of the few patients who have suffered in the manner last mentioned, the larger number were women, and these almost always of a more or less hysterical temperament. But it is nevertheless true, that cases have been recorded, in which the pain in the stumps, and the spasms, excited hysterical attacks which had never been developed before.

This affection is so distressing, and so little under the control of remedies, that surgeons have been at great pains to ascertain the exact nature of the neuromata or nerve-bulbs which sometimes follow amputation, in the hope that that knowledge might lead the way to a method of avoiding their formation. It has been found that the bulbs are not composed of nervous filaments, but that the bulk is made up of exudation matter, distributed among the fibrillæ of the nerves; and there are few stumps which do not contain some of these bulbs. But it is probable, that when they increase beyond a certain size, and are compressed by a hard cicatrix, they excite the pain and convulsive movements which are sometimes noticed. If this be true, a second amputation offers a good chance of freeing the patient from these symptoms; but herein we do not always succeed, as will be seen by the case lately under Mr. Hancock's care.

Nor are there examples wanting to show that there may be, in certain



subjects, such a confirmed propensity to this kind of neuritis, that the affection will spring up again and again, though the bulbous extremities may be removed, or even the whole stump taken off at the joint. In fact, pain and convulsive movements may occur without the formation of bulbs, the symptoms probably depending on the more or less nervous inflammation which attends the process of cicatrization. On this head Mr. Miller says, very justly: "In the case of neuralgia apparently dependent on neuromata, these may be taken away by incision, and the wound treated most carefully, yet the same painful feelings are very prone to return, before fresh neuromata have had time to form; and may continue, even when careful manipulation satisfies the surgeon that additional neuromata have actually not been produced." This will be very strikingly illustrated by the following case, which for several months excited a lively interest at the hospital. From notes taken by Mr. Dalton, the house-surgeon, we become acquainted with the following facts:—

Ann V——, aged twenty-nine, of fair complexion, blue and somewhat wild-looking eyes, was admitted April 10, 1851, under the care of Mr. Hancock. She is evidently of a highly nervous and excitable temperament; has generally attended to household duties, without any fixed occupation, and seems to be affected with the peculiar hysterical tendency of exciting as much as possible the attention and sympathy of those with whom she comes in contact. The left arm has been amputated, at about the middle point between the head of the humerus and the condyles, and the stump is constantly and convulsively moving, principally in a direction from above downwards. The pectoral muscles, the trapezius, the scaleni, and the platysma myoides, are also strongly agitated, and the parts are never at rest except when the patient is asleep.

She states that she was seized, about sixteen years ago, with severe pain and inflammation of the elbow-joint, (without having met with any injury,) and soon after, a rather serious illness, probably continued or other fever. For these symptoms she was actively treated; but becoming worse, she repaired to town, and placed herself under the care of the late Mr. Morgan, at Guy's Hospital. Here the usual means employed for diseases of joints were carefully used, but to no purpose; and Mr. Morgan was at last obliged to take off the arm—a period of five years having elapsed between the first onset of the articular disease and this amputation. The patient returned to the country before the stump was completely cicatrized; the healing process was completed in about two months, and a firm cicatrix formed.

At the expiration of three months after the operation, and one after the entire closure of the wound, the stump began to move convulsively, most of the muscles of the shoulder and neck connected with the arm soon partaking of the spasmodic twitchings. This had been preceded for several days by very intense pain in the extremity of the stump. Soon afterwards, a collection of matter occurred in the part, as she was then several times seen by the late Mr. Aston Key, at St. Alban's, who recommended her to return to Guy's Hospital. She now stayed in that

institution for several months, during which, blisters, setons, issues, ointments of various kinds, and general treatment, were tried with great perseverance, but without any relief to the convulsive action or to the pain. Among the means employed, was the tying down of the stump; but when the latter was well secured, and the movements completely prevented, the *whole body* used to be thrown into convulsions. The pain would sometimes radiate from the stump to the neck, back of the head, and down the spine, and convulsions occasionally were noticed in the left thigh, leg, and great toe. Even the right shoulder shook now and then violently, and became exquisitely painful. She often woke suddenly, and found both the left leg and the stump in violent convulsions, but the movements used mostly to cease during sleep. The patient, however, stated that she had often been told that the stump, even when she was sound asleep, would start occasionally. It was likewise noticed that, at the height of the convulsive action, the skin and cellular tissue of the cicatrix became violently retracted.

Some time after leaving Guy's, the patient was admitted into St. George's Hospital, where the series of remedies available and sometimes efficacious in such cases, were successively tried, but as none gave any relief, amputation of the shoulder joint was mentioned, and agreed upon, two years having elapsed since the removal of the arm. But at this time a new feature arose in the patient's case; severe epileptic fits set in, which occurred more or less frequently for the space of two years after she had left St. George's.

We now find that the girl fell into the hands of a mesmerist, and she states that the convulsive action generally stopped during the artificial sleep. But it is difficult to put any faith in the circumstances connected with this period of the patient's history, as everything coming from such quarters must be received with extreme caution. There can be no doubt, however, that the girl must have been made much worse by the mesmeric juggleries, and that her present nervous, hysterical, and excitable condition is principally owing to the constant irritation offered to her nervous system, during seven years mesmeric treatment. After this, the patient suffered so much pain and inconvenience, that she applied for admission at St. Bartholomew's Hospital, where she stayed about nine months. The best directed means were here again perfectly useless; the amputation at the shoulder-joint was proposed, but she seems to have been at that time unwilling to submit to this last resource. At St. Thomas's Hospital, whither the patient next repaired, Mr. Green and Mr. Travers were of opinion that there was no chance of any relief but by removal at the joint. Some incisions were here made into the stump, but she left without consenting to the operation proposed.

Frequent blisters were subsequently applied to the part, as well as to the spine; galvanism was tried, but all to no purpose. The violence of the fits would sometimes abate considerably, and then she improved in health. She has always exhibited signs of indolence, and within the twelvemonth which preceded her admission into Charing-cross Hospital she gained much flesh.

The patient has amused herself in casting up the number of some of



the remedies which were used in the different institutions where she was admitted, and finds that the stump was lanced twenty-five times, that she had four hundred and ninety-five leeches, ninety-six blisters, &c. It should be particularly noticed that during all these years, the catamenia (which had begun at eleven) were always very regular.

Mr. Hancock endeavored at first to give tone to the system, and to overcome the hysterical and nervous state of the patient. For this purpose, the most approved means, as quinine, steel, zinc, morphia, &c. &c. were used for a considerable period, but without any very tangible effect. It was clear, after several months had elapsed, that a more direct interference would be required to subdue the convulsive action of the stump. There could be hardly any doubt but that the bulbs of the nerves were much enlarged, and that their great size would ever be in the way of a cure; Mr. Hancock therefore resolved, before having recourse to the complete removal of the stump, to dissect out the bulbous extremities of the nerves.

On the 22d of August, 1851, whilst the patient was under the influence of chloroform, Mr. Hancock made a longitudinal incision, from the shoulder downwards along the anterior aspect of the stump. When the parts were fairly laid open, a bulbous mass, about the size of the last phalanx of the thumb, came into view. This was carefully freed from the surrounding parts, down to the bone, and removed. During the operation the brachial artery was divided, and immediately tied; the lips of the wound were brought together, and when the stump was placed upon the pillow, it was observed that the spasmodic movements had entirely ceased. This circumstance was extremely satisfactory, and confirmed the belief that the presence of the nervous bulbs had been mainly instrumental in the persistence of the symptoms through so many years.

This favorable state of things lasted about one month; the stump lay perfectly still, and hopes were entertained that this very obstinate affection was at last conquered; but the patient, having been seized by a fit of impatience, about four weeks after the last mentioned operation, the stump began again to be convulsed, and matters soon became as bad as ever. It was now evident that nothing short of amputation at the shoulder-joint would be of any avail. Mr. Hancock, therefore, proposed the operation to the girl, who readily consented. On the 12th of November, 1851, the patient having been narcotized with chloroform, Mr. Hancock performed amputation at the joint, making a good flap of the deltoid muscle. It was noticed that the stump, when severed from the body and placed on the table, continued to be convulsively agitated for several minutes. No pathological appearance of importance was revealed by the examination of the stump, except that the nervous trunks were closely adherent to the bone.

The chloroform affected the patient to a very great degree, for about twenty-four hours after the operation, the hysterical diathesis having a large share in the somewhat alarming symptoms which were observed. This doubtful state did not, however, last long; the patient progressed very favorably for several weeks, and it might now be fairly surmised that all the pain and inconvenience which had been undergone for so

many years, were arrested; but hardly had the cicatrix formed, about two months after the operation, when the muscular fibres forming the deltoid, those of the trapezius, the scaleni, a portion of the great pectoral, and even the platysma myoides, were again thrown into strong convulsive action. The movements were just as continuous and spasmodic as they had been in the stump. When the girl was not observed, the jerking was much less rapid than when she was either angry or excited. Some suspicion had previously existed, and was now made somewhat strong, that the movements were, perhaps, not completely involuntary, Mr. Hancock thought it now prudent to allow the girl to breathe the country air; she was therefore discharged, February 15th, 1852, about ten months after her admission.

This is certainly one of the worst cases of the kind which have as yet been put upon record, with the only exception that Mr. Hancock's patient had no aneurismal pulsations accompanying the convulsive action. This case fully confirms the very generally received opinion, that neuralgia and convulsive movement of stumps, depending on a bulbous state of the extremity of the divided nerves, are hardly amenable to treatment, and that even the very severest measures, such as a complete removal of the stump, are found inefficacious. That the hysterical diathesis so unmistakable in Mr. Hancock's patient, must have had a very large share in the production of the symptoms, cannot for a moment be doubted; but it is clear, on the other hand, that the extremities of the nerves were more bulbous than is the case in the generality of stumps, and that there was a material and tangible cause for the symptoms besides the highly nervous and hysterical temperament. Had the patient never been affected with the disease of the elbow joint, which led to the first amputation, it is very likely that the hysterical diathesis, which exercised its full force on the stump, would have manifested itself in one of the numerous ways belonging to hysteria.

We need not refer our readers to the paper of Mr. Langstaff, in the sixteenth volume of the *Medico-Chirurgical Transactions*, to show that the soundness of a stump depends very much on the manner in which the soft parts cicatrize, and that very severe symptoms are apt to ensue when the bulbs of the nerves, which form almost in every stump, are increased to such an extent as to assume a neuromatous character. As to the actual nature of the enlargement, a subject to which we alluded above, Mr. Langstaff says:—"I think it right to mention, that on cutting through these bulbous extremities of the nerves, there are no signs of enlargement of their natural structure, the thickening appearing to have been occasioned wholly by the deposition of lymph, the effect of inflammation in the cellular tissue covering the neurilemma."

Mr. Langstaff mentions, in the same paper, the case of a girl, twenty years of age, whose forearm had been taken off, and who, like Mr. Hancock's patient, suffered from pain and convulsive action in the stump, with the addition of aneurismal pulsation. Every kind of means was tried, at several hospitals, without any favorable results; at last Mr. Langstaff took off the arm above the elbow-joint; and, in describing the



operation, he says:—"I drew out each nerve, to the extent of half an inch, from the surface of the stump, with a tenaculum, and cut through them to prevent their interrupting the progress of cicatrization, which I have done in other instances with success; and I was astonished that the division did not occasion greater pain than was complained of by the patient." "This patient was relieved of all the painful sensations she had so long been distressed with; she had no recurrence of hysteria, (which affection had been induced by the convulsions of the arm,) her health improved; a good stump was effected; and she is now (1830) able to obtain her livelihood." We have some reason to suspect that this same patient had, some time after the publication of the paper, a relapse; she then came under the care of Mr. Tyrrel, who performed amputation a little higher up, without effecting a cure. Mr. Bransby Cooper lastly removed the stump at the joint, and at this moment the soft parts about the shoulder are as convulsively agitated with this woman, as is the case with Mr. Hancock's patient.—*London Lancet*.

### OBSTETRICS.

PARTURITION AMONG THE ESQUIMAUX.—At a recent meeting of the Obstetrical Society, Dr. James Struthers exhibited the pelvis of a female Esquimaux, which was chiefly remarkable for the unusually large dimensions of the brim, cavity, and outlet, allowing the largest foetal head to pass with great ease in almost any direction. The following are the diameters of the brim:—Transverse, 6 inches; conjugate,  $4\frac{3}{8}$  inches; oblique,  $5\frac{3}{4}$  inches. Of the cavity:—Transverse,  $5\frac{1}{4}$  inches; conjugate,  $5\frac{5}{8}$  inches; oblique,  $5\frac{1}{2}$  inches. The transverse diameter of the outlet was  $5\frac{1}{8}$  inches; and the public arch was unusually wide.

Dr. Struthers received the pelvis from Mr. G. C. Pirie, who found it last summer under a cairn of stones, (the common mode of burying among the Esquimaux,) near Cape Hooper, in Davies' Straits. It is an interesting fact, (of which Mr. Pirie satisfied himself, thereby confirming the statements of various writers on Greenland,) that the Esquimaux women have much safer and more rapid labors than the women of this country. There the whole process of parturition is left in the most absolute manner to nature. As soon as the woman begins to complain, she retires to a low skin hut, built for the purpose, into which no one is allowed on any account to accompany her. Before the lapse of an hour, she generally makes her appearance with the baby on her back. Not unfrequently labor comes on unexpectedly. Such a case came under Mr. Pirie's own observation. The woman left the ship, retired behind a block of ice, and came back within half an hour with the child in her hood. It is stated that the cord is not divided until the placenta has come away, and that the division is effected by the teeth of the mother. Miscarriages, twins, and monsters are of rare occurrence, and it is a very uncommon thing for a woman to die in childbed.—*Medical Times and Gazette*, May 8th, 1852.

*On the meaning of the term "Ulceration," as at present applied to uterine diseases.* By T. SNOW BECK, M. D., F. R. S., Physician to the Farrington General Dispensary and Lying-in Charity.—The conflicting opinions which have been expressed by practitioners as to the absence or presence of ulceration at the orifice of the uterus, appear only to be accounted for by referring to the different senses in which this word has been used. At first sight it appears strange, that two opinions should exist as to the meaning of a word the definition of which is so generally agreed upon; yet it is, nevertheless, certain, that in regard to the diseases of the uterus, it has been employed to denote morbid conditions which are very different from ulcerations, in the received acceptation of the term.

The definition given by all surgeons is essentially the same. Thus, for example, Boyer states: "An ulceration is a solution of continuity of the soft parts, more or less ancient, accompanied by a purulent secretion, and kept up by some local or internal cause;" and Samuel Cooper says: "Surgeons usually define an ulcer to be a solution of continuity in any of the soft parts of the body, attended with a secretion of pus, or some kind of discharge." Authors on practical medicine and pathology likewise adopt a similar definition. Dr. Copland considers "ulceration is distinguished from suppuration chiefly by its being attended by a loss of substance, by a destruction of parts, and by a more or less abundant secretion of puriform, ichorous, fetid, sanious, or variously-colored fluid." Dr. Watson writes: "We may say, with Dr. Allison, that, whenever the absorption of the effused lymph, and of the surrounding textures, takes place in excess—in a greater degree, that is, and more irregularly than seems to be required for any useful purpose—the result is *ulceration*. The term is, however, commonly restricted to those cases in which the loss of substance occurs upon some *surface*, internal or external." Dr. Williams defines an ulcer to be "a solution of continuity or excavation, the bottom and edges of which continue to discharge pus, or a serous fluid mixed with exudation corpuscles, and sometimes blood corpuscles." Chomel states, that "ulcers are solutions of continuity maintained (*entre tenues*) by an internal or local disease." Andral says, "The term ulcer is applied to that solution of continuity which is produced in a tissue by the absorption of its molecules."

According to these definitions, to which many more might be added, the essential character of ulceration is a solution of continuity or excavation, or a loss of substance in a part; for it is evident, that the discharges which usually accompany this lesion are not characteristic of, nor essential to it, inasmuch as they are also products of simple inflammation of a mucous membrane. The accuracy of these definitions are confirmed by more recent observation of morbid phenomena by the aid of the microscope, which has shown, that, previous to ulceration taking place, the blood becomes stagnant, and adheres to the walls of the vessels, that the neighboring tissues thus lose their vitality, by changes similar to what occur in mortification, and, being carried away by effused fluid, a solution of continuity, or loss of substance, is produced. To express this series of morbid changes in a short phrase, many patholo-



gists define ulceration as "molecular death," or "molecular gangrene." But this does not in any way alter the essential character of the previous definitions: for, when "molecular death or gangrene" occurs in any tissue, "a solution of continuity, or loss of substance" in that tissue is the inevitable result.

Admitting, then that these definitions correctly express the essential character of ulceration, we may inquire, whether this term has been applied with propriety to many morbid states of the orifice of the uterus. At the late discussion at the Royal Medical and Chirurgical Society, Dr. Murphy is reported to have said: "A girl was admitted into the hospital (University College) dying from the effects of opium. At the autopsy, he had examined the uterus, and found an ulcer on both lips extending into the cavity. It was seen by the pupils, and was sent to the curator to be preserved, but the next day it had lost all its characters, and was thrown aside." With all respect for Dr. Murphy's high standing in the Profession, and the personal esteem which must be felt by all who know him personally, I can only come to the conclusion, that some source of error must exist in this statement. A solution of continuity, or a loss of substance, on the lips of the uterus, would have been as readily recognised the day after the autopsy as on the day of the examination; and, if the supposed ulceration "had lost all its characters the next day," is it not certain that ulceration had not existed, and that some other morbid appearance had been mistaken for it? How could the loss of substance be repaired? And, if still present, it would have been recognised. I feel the greater confidence in speaking upon this subject, since I have carefully searched during some years for these reputed ulcerations in my own practice, and in the hospital practice of other physicians, without being enabled to discover them. During this period, I have seen many cases of inflammation of the mucous membrane covering the lips of the uterus, and even extending into the cervical canal, which presented a red and granular appearance, and which had been designated ulceration, but wherein no loss of substance or solution of continuity existed. Sometimes the glands in the mucous membrane were chiefly implicated in the inflammation, and each being surrounded by a defined circle of redness, they presented, when grouped together, a circumscribed and defined granular patch, studded with minute pits or holes, which led to the orifices of the glands. In these cases, the pathological condition did not differ from that of chronic inflammation in other mucous membranes, as, for example, in the granular condition of the conjunctiva; in the state of the mucous membrane of the stomach, to which the name of mammillation has been applied, in the condition sometimes observed in the urethra of the male after gonorrhœa, etc. In these instances, there is no loss of substance in the membranes, nor has any one contended that ulceration was present; yet this name has been applied to the same morbid conditions when observed in the mucous membrane of the vagina, or in that covering the lips of the uterus. With this explanation, it can readily be understood, that an inflamed mucous membrane might lose its characteristic appearances the day after the autopsy, especially if it had been immersed in any fluid; but if a

loss of substance or an ulcer had existed, that ulcer would have been as readily discovered the next day, as on the day on which the first examination was made.

At the same meeting of the Society, a letter was read from Mr. Holl, detailing the result of his *post-mortem* examinations, and bearing upon this subject. Dr. H. Bennett afterwards remarked: "That it appeared to him that Mr. Holl's guarantee for the correctness of his opinions was all that could be desired, as he was practically acquainted with the lesions to which that part is subject in the living state; that he had carefully examined the condition of the parts, and stated in his letter,—'The whole number of bodies in which the state of the uterus was examined was 44; of these 34 either presented no disease of the cervix uteri, or only slight congestion, with the exception of 3, in whom the congestion was considerable. Of the remaining 10 cases, 9 of them presented more or less extensive abrasion of the epithelium from around the os uteri, but no ulceration. The pelvic veins were generally full of blood; the cervix uteri large, swollen, and puffy, with more or less venous congestion of the mucous and sub-mucous tissues, but no induration or effusion of lymph into its texture. The glands of Naboth were enlarged, and the canal of the cervix blocked up with viscid mucus. The removal of the epithelium appeared to commence at the follicles which surrounded the cervix uteri.'" Dr. Bennett, having read the letter, drew attention to the fact, that Mr. Holl, having a knowledge of these special lesions, and having specially investigated them in the autopsies, discovered 10 cases out of 44, or 37 per cent., in which there existed abrasion, or ulceration of the uterine mucous membrane,—conditions which he (Dr. Bennet) considered pathologically identical. This, however, appears a perversion of Mr. Holl's words, who distinctly says, "there was abrasion of the epithelium, *but no ulceration*." After lauding Mr. Holl's knowledge of these special lesions as offering a guarantee,—all that could be desired for the correctness of his opinions,—it appears a singular proceeding to ignore his knowledge, and contradict the opinion founded upon it. Would it not have been more consonant with the facts to have stated, Mr. Holl said he had found nine cases of abrasion of the epithelium, but no ulceration; yet this statement was erroneous, as, in reality, there were cases of ulceration? And that this conclusion was come to, in opposition to the former praise, because abrasion of the epithelium and ulceration were considered conditions pathologically identical. But is this the fact? Are these lesions pathologically identical?

The essential character of ulceration—that it is a solution of continuity or loss of substance in a tissue—has been sufficiently proved, and the pathological changes which occur have also been pointed out. In abrasion, or excoriation, the epithelial covering of the mucous membrane is removed, but the substance of the membrane itself remains uninjured. There is no solution of continuity, or excavation, or loss of substance in the part, and this constitutes the essential difference between the two lesions—abrasion, or excoriation, and ulceration. The pathological changes which occur are equally distinct. In ulceration, the blood be-



comes stagnant, adheres to the sides of the vessels, the vitality of the part is destroyed, molecular death or gangrene being induced. In excoriation, the blood flows on without interruption or stagnation; a portion of serum is effused beneath the unorganized epithelium, which, being detached, is subsequently removed. The changes which occur may be compared to the effect of an exceedingly minute and mild blister, active in its character, acute in its course; whilst ulceration is the consequence of minute mortification, usually slow in its action, chronic in its course. The difference between the two lesions, and the pathological changes which occasion them, thus appear to be well defined; but there is one point of view from which the two might be confounded. The first change occurring in a mucous membrane about to undergo minute gangrene is, in some cases, the effusion of bloody serum beneath the epithelium, and the subsequent desquamation of this part. And if we were to inquire no further into the changes going on, it might be concluded that, as there is desquamation of the epithelium in the beginning of the process of ulceration, and also in excoriation, that hence the two states are pathologically identical. But this would be a very superficial mode of considering the question. In the changes which produce ulceration, desquamation of the epithelium is not the only alteration which takes place in the membrane; for, after its removal, the surface appears livid and dark-colored, the tissues having lost their vitality, and the vessels of the part being blocked up, as in mortification. But in excoriation, the separation of the epithelium is the first and only change which occurs in the tissues of the membrane, which, after its removal, appears red and florid, the vessels being pervious, and the blood circulating through them. Excoriations, then, are the consequence of inflammation of the mucous membrane, or, as some have expressed, of catarrhal inflammation, more or less acute; ulcerations are the results of molecular gangrene, or mortification, the sequence of disease extending into the deeper tissues of the organ. Thus, even from this point of view, it would be illogical and erroneous to consider these two morbid conditions as pathologically identified.

The importance of this subject is greatly increased when considered in relation to the treatment of the diseases of the uterus. From many communications I have received, I have no doubt that the idea of having so loathsome a disease as "ulceration" on the uterus, has induced many females to submit to the employment of the speculum, and escharotics; while the same term has been considered by practitioners as a justification for this mode of treatment. It is thus intimately connected with the proper or improper use of the speculum, in the treatment of uterine disease. Where ulceration really exists, which is very rare, unless it be specific ulceration, the local application of escharotics by the aid of the speculum, during the later stages of the disease, is, no doubt, an useful adjunct to the other means of cure; but when excoriation only is present, or a reddened and swollen state of the mucous membrane, conditions which accompany inflammation more or less acute, the employment of these therapeutical agents is not only improper, but frequently most injurious.

It appears to have been overlooked in the discussions on this subject, that neither excoriations nor ulcerations are, in themselves, the essential disease; but that they are merely symptoms or evidences of the existence of other diseases which have produced these morbid states. This is well shown in Mr. Holl's account of the morbid appearance, where "the pelvic veins were generally full of blood, the cervix uteri large, swollen, and puffy, with more or less venous congestion of the mucous and submucous tissues, but no induration or effusion of lymph into its texture. The glands of Naboth were enlarged, and the canal of the cervix blocked up with viscid mucus,"—distinct evidences of inflammation of the substance of the organ,—while "the removal of the epithelium, which appeared to commence at the follicles surrounding the cervix uteri," was but a sign of this inflammation affecting the glands imbedded in the tissues of the part. Viewed in this light the application of escharotics to remove a symptom, whilst the disease producing it is unheeded, is undoubtedly not the correct course to pursue; nor can it be correct to endeavor to remove the symptom of acute inflammation of the mucous membrane—excoriation—by the application of substances which increase the inflammation already present. This mode of designating a reddened and swollen mucous membrane an ulceration, and including in one group diseases which are pathologically distinct, and which require opposite methods of treatment, has been productive of a serious amount of mischief in the treatment of uterine affections. Thus, in subacute inflammation of the mucous membrane of the vagina, where the membrane is red and swollen, and where the symptoms were, pain deep in the hypogastrium, pain on passing the feces or urine, a mucous or purulent vaginal discharge, with pain across the forehead, and other constitutional symptoms, I have known the speculum employed, and the nitrate of silver applied, in consequence of the practitioner being misled by the idea that extensive ulceration existed. As might be supposed, this treatment was followed by great increase in the symptoms for several subsequent days, without any permanent advantage. On the other hand, in some exceptional cases of chronic vaginitis, which chiefly involves the upper part of the vagina, and where lotions have been inefficiently employed, either from inattention or want of management on the part of the female, the local application of strong stimulants has altered the morbid action, removed the thickened or mammillated condition of the membrane, subdued the vaginal discharges, and other symptoms. But these are only exceptional cases, and, by no means form the general rule. Similar errors have been committed in respect to the diseases of the uterus itself. In cases of inflammation of the organ, the redness of one of the lips, or the red appearance of the lining mucous membrane, have been treated by the application of escharotics, and followed by much augmentation of the inflammatory action in the deeper tissues. Even in those cases where ulceration, or a loss of substance, has really existed, much injury results from concentrating the attention too much upon this lesion, and neglecting the diseases or morbid states to which it owes its origin. What surgeon would think of applying powerful local applications to an ulcer on the leg, in the



expectation of curing it, until he had first removed the state of the venous circulation upon which the continuance of the ulcer depends? Yet this is exactly what has been continually done in the treatment of diseases of the uterus.

These principles were well expressed by Professor Dubois, during the discussion before the Academy of Medicine at Paris, in 1850: "The value which has been given by one of our colleagues to ulcerations, if this were real, leads to consider them as one of the local conditions the most worthy of attention in practice. I cannot, for my part, adopt this manner of view; according to my appreciation, these lesions, as well as "engorgements," are only consecutive phenomena of another alteration (inflammation) which has preceded them, and have in general in regard to the symptoms and treatment, but a secondary importance."—*Bulletin.* *London Lancet.*

### ANATOMY AND PHYSIOLOGY.

*On the Occasional Organic Union of Contiguous Teeth.* By S. J. A. SALTER, M. B., etc. (Communicated by THOMAS BELL, Esq., Sec. R. S.)—The object of this paper is to enumerate fresh examples of the so-called "bony" union of contiguous teeth; to explain the true nature of that union, as exhibited by microscopical scrutiny; and to suggest the probable mode of its formation.

1. The fresh instances here recorded are, a united dens sapientiae and supernumerary tooth of the lower jaw; a cuspidatus and lateral incisor of the lower jaw united; and a specimen exhibiting the lateral and central incisors of the lower jaw in the same condition. These are particularly described.

2. In the next place, the nature of the uniting medium is shown to be dentine and not crusta petrosa, (as stated previously by writers on this subject.) This is demonstrated by microscopical drawings of sections of the teeth.

3. The probable mode by which this abnormality is produced is explained thus:—The pulps of the teeth form in their usual place and usual relation, with this one exception, that the portion of the capsule which should separate them is wanting. The result is, that they come in contact, and when the soft dentinal elements commence calcifying they unite, and the tubes of the two teeth become continuous. There is no enamel between the teeth where the crowns are in contact. The author further remarks, that all the teeth, and in both jaws, have been subjects of this peculiar condition. The paper concludes with a few practical remarks—that, in the majority of cases, the union could not be discovered while the teeth were in the mouth; and that, even where it could be discovered, both teeth must necessarily be removed together, should extraction of either be required, as there appears no means of separating them.

Mr. Streeter would ask whether these teeth might not be examples of duplex formation, like the double uterus, etc., and not the result of union. The inscription on the drawing sent round, certainly describes

the teeth as the bicuspid and the incisor, but they looked like a duplex formation, and he did not see why that process should not occur in the teeth.

A Visiter thought, that if such were the case, the regular number of the teeth would be imperfect, which did not in this instance appear to be the case.—*Trans. Royal Med. and Chir. Society in London Medical Times.*

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*On the molecular origin of the tissues.* By Dr. BENNETT.—The great generalisation of Schwann was that all tissues are derived from cells. Subsequently, it was ascertained that the nucleus, or cell-germ, exercised an influence on the tissues, independent of its cell wall; and it was endeavored to be shown, that some tissues might be derived directly from nuclei. The object of this communication was to point out that the nuclei themselves originated from smaller bodies,—viz., molecules; that these were the origin of every texture, and to indicate some of the laws which governed their formation, arrangement, and subsequent development. From a review of the observations of Schleiden, Schwann, and Martin Barry, the author pointed out how the first appearance, observable in all developing organisms, was a mass of molecules and granules, which, by aggregating or melting together, constituted the cell-germ. Around the cell-germ other molecules were formed, which again, by melting together, constituted the cell-wall. Further development, in like manner, proceeded by the apposition of molecules. At any period in the process of evolution, the onward progress might be checked when the structure became disintegrated in the inverse manner to its formation: First the cell wall became dissolved, then the nucleus, both of which were reduced, first to molecules then to a fluid. Hence there were molecules of evolution and molecules of disintegration. Occasionally, between the cell wall and nucleus, secondary molecules were formed, which constituted peculiar secretions, as they have been termed. These might be called molecules of transformation.

The author described the origin and mode of formation of these three kinds of molecules, their physiological and pathological importance, and pointed out the advance which had been made in our knowledge of molecular formation by the observations of Ascherson, Harting, and Melsen.

In complex organisms, the higher tissues were formed by an elaboration of blastema, mainly due to the successive evolution, transformation, and disintegration of matter, by means of the different kinds of molecules, of which the author gave numerous examples, derived from the elaboration of the ovum, of the blood, the transformation of insects, the process of fissiparous division in the lower animal forms, etc. He pointed out that molecules had independent movements, sometimes physical, as in the case of Brown's molecular movements, at other times vital, as seen in many organisms. That occasionally we had molecular fibres, from the aggregation end to end of molecules, in the same way as we have nuclear cell fibres. Moreover, each kind of fibre could assume inherent contractility, as in the case of vibriones, which might be called contractile molecular fibres, as spermatozoa might be denominated contractile nuclear, and cilia contractile cell fibres.—*Lond. Monthly Jour.*